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HOW TO GET STRONG.

[The preceding parts of this series appeared in Nos. 32, 33, 35, 36, 41, 44, 49, 53, 58, 65, 66, 90, 91, 92, 94, 95, 97, and 104.]

AS the series of papers under this heading has been considerably interrupted, through increasing pressure on the Editor's available space, it seems desirable, before resuming it, to give a brief *résumé* of preceding portions, noting in what numbers they are to be found, and pointing out were lie the threads which we now intend to take up and write into the remaining parts of the series, so that when finished each part of our subject may be found to have been fully dealt with, however irregular the treatment of the series may seem to have hitherto been.

In No. 32, June 9, 1882, we pointed out how necessary it is that all men engaged in the struggle of life should maintain the vigour of their limbs and muscles and internal organs, but how brief is the time which busy men can usually afford for the purpose. But we showed that men do not need to be athletes to have their bodies in due working order, prepared to resist any such sudden strain as may at any time tax its energies, and find out a weak spot if any such there be.

In No. 33, for June 16, 1882, we began the detailed investigation of those exercises which are calculated to retain the vigour of particular muscles and organs. We started with the chest, dwelling on the value for chest development of sparring (not necessarily or even desirably with a living and breathing opponent), and of systematic inhalation and exhalation to the full extent of the lungs,—giving an illustrative instance of the great improvement in chest compass which such exercises can impart.

In No. 35, for June 23, 1882, we dwelt further on the importance of systematic exercise in inhalation and exhalation, and in answer to numerous correspondents, touched on the bad effects which any single form of exercise, however good in itself, must inevitably have on symmetry and general activity, if pursued without due admixture of other exercises. We touched there on a circumstance which any one who chooses to inquire into may readily test during the approaching season, the unsymmetrical figures of rowing men who are rowing men only, cricketers and lawn-tennis players who care for no other exercises, runners, tricyclists, and so forth who practise only their own favourite exercise, "only that and nothing more."

In No. 36, for July 7, we dwelt further on exercises for expanding the chest, dwelling in particular on rope climbing (for the young and light) and bell-ringing, or rather the kind of pull that is involved in bell-ringing. In No. 41 we added a form of exercise especially intended to expand and deepen the chest.

In No. 44, for Sept. 1, we turned our attention from the enlargement of the chest itself to the fortifying of the muscles outside the chest. We dwelt on the importance of distinguishing the matter of mere muscular development around the chest, a desirable result, from the essentially vital points,—1st the development of the frame enclosing the breathing organs, and 2ndly the development of these breathing organs themselves. We had rightly made the expansion and deepening of the chest itself and the training of the organs it contains to more complete fulfilment of their vitalising work, take precedence of the development of the pectoral and dorsal muscles. In fact, we pointed out that many exercises which increase these muscles and give to the chest (at least when examined by inexperienced eyes) an appearance of splendid development, positively injure the chest and act as a drain on the vitality instead of increasing it.

However, strength in the pectoral muscles is a good thing in itself, though not so necessary for men as it is for birds. We described several exercises which serve to strengthen them greatly, as (for the young and light) work on the parallel bars, work in thrusting downwards from the shoulders towards the knees, and slow work in slowly lowering dumb-bells held with the arms extended horizontally on either side and as slowly raising them again,—exercise requiring but a few seconds to do its work.

In No. 49, for October 6, 1882, still dealing with chest muscles but now considering the dorsal ones we dwelt on those exercises for the upper back and shoulders which are usually taken rather in excess than insufficiently in this country. Rowing in particular which is supposed by many to expand the chest and strengthen the upper arm muscles, in reality strengthens most the shoulder muscles, (next the leg muscles, and last the lower arm muscles,—not strengthening the upper arm muscles at all). It is well to notice this because it has been proved by measurement that if rowing is not corrected by other forms of exercise it absolutely tends to contract the chest.

In No. 53, for November 10, 1882, we gave an illustration of the ill-developed chest (especially as seen in the case of a rowing man (Fig. 1, p. 386, Vol. II.), and the symmetrical form given to the chest by ancient Greek sculptors working on the models supplied them in the gymnasium, where well-selected exercises gave well-proportioned figures to nearly every healthy Greek.

Next in No. 58, for December 8, 1882, we dealt with the muscles of the waist, wofully neglected in this country and therefore more liable to strain and accident generally than any muscles of the body. We showed how walking exercise should be taken, and what particular style of walk is good for weak waist muscles. In running also these muscles are exercised if the shoulders are well thrown back, the body upright, the strides long and energetic. We gave also some indoor exercises, requiring no apparatus, by which the waist muscles may be very efficiently strengthened.

In No. 65, for Jan. 26, 1883, we considered the mischievous effects of stays on the muscles of the waist, which in many cases lose all their strength and may be regarded as in part atrophied under constant corset pressure. In the

following number we dealt further with this important, one may say vital question, considering that though few men are so foolish as to ruin their abdominal muscles this way, all who are "born of women," which includes a considerable proportion of the human race, may inherit weakness thus occasioned.

In No. 90, for July 20, 1883, the series of papers on "How to Get Strong," which had been for a long time discontinued, was resumed. But we did not take up the subject where we had left it off. We had only dealt, thus far, with the chest and waist, and still had the important muscles of the arms and legs, besides the muscles of the hands, feet, and neck to deal with, before proceeding to consider the exercise of the body as a whole, and such questions as diet, regimen, training, and so forth. In No. 90, however, we commenced a long digression on the subject of reducing fat. This was continued in Nos. 91, 92, 94, and 95.

Lastly, in Nos. 97 and 104, we dealt with the question of such exercise as is suitable in advanced and middle life, a subject to which, as it is of extreme importance we shall refer later on, and also, *pro re natâ*, in treating of specific exercises for developing strength and lissomeness in particular muscles.

Here our survey of the past numbers ends. We resume the series at the point whence we digressed in No. 90. We had then dealt with the development of the muscles of the chest, (pectoral and dorsal), and waist. We have still some further remarks to make on the important and much neglected muscles of the waist and abdomen. Then we shall consider the muscles of the legs, before proceeding to those of the arms, because the muscular development of the legs has been more neglected than that of the arms; hundreds who by the use of dumb-bells, Indian clubs, and so forth strengthen their arms effectively enough, leave their legs with no exercise but walking and dancing, though at the very time that they are swinging clubs or bells they might be giving their lower extremities excellent and interesting occupation, making them at once stronger, more lissome, and more symmetrical. Besides which, owing to the circumstance that the legs have to support the weight of the body, exercises without apparatus are more easily devised for the legs than for the arms.

(To be continued.)

GAMBLING SUPERSTITIONS.

By RICHARD A. PROCTOR.

(*Continued from page* 108.)

BUT we may learn a further lesson from our illustrative tossers. We have seen that granted only a sufficient number of trials, runs of luck are practically certain to occur; but we may also infer that no run of luck can be *trusted* to continue. The very principle which has led us to the conclusion that several of our tossers would throw twenty "heads" successively, leads also to the conclusion that one who has tossed "heads" twelve or thirteen times, or any other considerable number of times in succession, is not more (or less) likely to toss a "head" on the next trial than at the beginning. *About half*, we said, in discussing the fortunes of the tossers, would toss "head" at the next trial—in other words, *about half* would fail to toss "head." The chances for and against these lucky tossers are equal at the next trial, precisely as the chances for and against the least lucky of the ten million tossers would be equal at any single tossing.

Yet, it may be urged, experience shows that luck continues; for many have won by following the lead of lucky players. Now I might, at the outset, point out that this belief in the continuance of luck is suggested by an idea directly contradictory to that on which is based the theory of the maturity of the chances. If the oftener an event has occurred, the more unlikely is its occurrence at the next trial—the common belief—then contrary to the common belief, the oftener a player has won (that is, the longer has been his run of luck), the more unlikely is he to win at the next venture. We cannot separate the two theories, and assume that the theory of the maturity of the chances relates to the play, and the theory of runs of luck to the player. The success of the player at any trial is as distinctly an event—a chance event—as the turning up of ace or deuce at the cast of a die.

What then are we to say of the experience of those who have won money by following a lucky player? Let us revert to our coin-tossers. Let us suppose that the progress of the venture in a given county is made known to a set of betting men in that county; and that when it becomes known that a person has tossed "head" twelve times running, the betting men hasten to back the luck of that person. Further, suppose this to happen in every county in England. Now we have seen that these persons are no more likely to toss a thirteenth "head," than they are to fail. About half will succeed, and about half will fail. Thus about half their backers will win and about half will lose. But the successes of the winners will be widely announced; while the mischances of the losers will be concealed. This will happen—the like notoriously does happen—for two reasons. First, gamblers pay little attention to the misfortunes of their fellows: the professed gambler is utterly selfish, and moreover he hates the sight of misfortune, because it unpleasantly reminds him of his own risks. Secondly, losing gamblers do not like their losses to be noised abroad; they object to having their luck suspected by others, and they are even disposed to blind themselves to their own ill-fortune as far as possible. Thus, the inevitable success of about one half of our coin-tossers would be accompanied inevitably by the success of those who "backed their luck," and the successes of such backers would be bruited abroad and be quoted as examples; while the failure of those who had backed the other half (whose luck was about to fail them), would be comparatively unnoticed. Unquestionably the like holds in the case of public gambling-tables. If any doubt this, let them inquire what has been heard of those who continued to back Garcia and other "bank-breakers." We know that Garcia and the rest of these lucky gamblers have been ruined; they had risen too high and were followed too constantly for their fall to remain unnoticed. But what has been heard of those unfortunates who backed Garcia after his last successful venture, and before the change in his luck had been made manifest? We hear nothing of them, though a thousand stories are told of those who made money while Garcia and the rest were "in luck."

In passing, we may add to these considerations the circumstance that it is the interest of gaming-bankers to conceal the misfortunes of the unlucky, and to announce and exaggerate the success of the fortunate.

I by no means question, be it understood, the possibility that money may be gained quite safely by gambling. Granting, first, odds such as the "banks" have in their favour; secondly, a sufficient capital to prevent premature collapse; and thirdly, a sufficient number of customers, success is absolutely certain in the long run. The capital of the gambling public doubtless exceeds collectively the capital of the gambling-banks; but it is not used collec-

tively: the fortunes of the gambling public are devoured successively; the sticks which would be irresistible when combined are broken one by one. We leave our readers to judge whether this circumstance should encourage gambling or the reverse.

It is also easy to understand why in the betting on horse-racing in this country and others, success ordinarily attends the professional bettor, rather than the amateur,—or, in the slang of the subject, why "the ring" gets the advantage of "the gentlemen?" Apart from his access to secret sources of information, the professional bettor nearly always "lays the odds," that is, bets against individual horses; while the amateur "takes the odds," or backs the horse he fancies. Now, if the odds represented the strict value of the horse's chance, it would be as safe in the long run to "take" as to "lay" the odds. But no professional bettor lays fair odds save by mistake. Nor is it difficult to get the amateur to take unfair odds. For "backing" is seemingly a safe course. The "backer" risks a small sum to gain a large one, and if the fair large sum is a little reduced, he still conceives that he is not risking much. Yet (to take an example), if the true odds are nine to one against a horse, and the amateur sportsman consents to take eight to one in hundreds, then, though he risks but a single hundred against the chance of winning eight, he has been as truly swindled out of ten pounds as though his pocket had been picked of that sum. This is easily shown. The total sum staked is nine hundred pounds, and at the odds of nine to one, the stakes should have been respectively ninety pounds and eight hundred and ten pounds. Our amateur should, therefore, only have risked ninety pounds for his fair chance of the total sum staked. But he has been persuaded to risk one hundred pounds for that chance. He has therefore been swindled out of ten pounds. And in the long run, if he laid several hundreds of wagers to the same amount, and on the same plan, he would inevitably lose on the average about ten pounds per venture.

In conclusion, I may thus present the position of the gambler who is not ready to secure Fortune as his ally by trickery:—If he meets gamblers who are not equally honest, he is not trying his luck against theirs, but, at the best (as De Morgan puts it) only a part of his against more than the whole of theirs: if he meets players as honest as himself he must, nevertheless, as Lord Holland said to Selwyn, "be—in earnest and without irony—*en verité le serviteur très humble des événements*, in truth the very humble servant of events."

THE EVOLUTION OF FLOWERS.

By GRANT ALLEN.

III.—INTEGRATION BEGINS.

BESIDES the true Alismas with which we have hitherto dealt, there are a few other Alisma-like plants in Britain, each of which helps us on a little way toward the development of the true lilies; and as it is better, where possible, not to travel beyond the limits of fairly well-known or easily obtainable flowers, we may as well take these English species as the representatives of the various intermediate stages.

In a very few spots among the South-eastern counties there grows a rare water-side weed of wet ditches or pools, known to botanists as *Damasonium stellatum* or *Actinocarpus stellatus*. It is a south European plant, which only just reaches our shores where they lie nearest to the Continent, and has never been able to spread itself further north or west, against the adverse climate and the stout competition of our more northerly waterside weeds. Damasonium at first sight presents a great many points of resemblance to the water-plantain; it has three small green sepals, three much larger white petals, six loose stamens, and a group of carpels in the bossy centre. Being fertilised by the same sort of flies as water-plantain, it has even the yellow spot at the base of the petals as in Alisma, which marks the way to the honey, and points back to the original yellowness of the whole petal. But when we come to look more closely into the flower, we see that it possesses two distinct symptoms of advance in organisation, each of which is very important as leading onward and upward in the direction of the true lilies. In the water-plantain we saw that the carpels were very numerous, sometimes as many as thirty in a single flower: but in Damasonium they are always six in number, that is to say, they are reduced to two whorls of three carpels each. The significance of this change is best seen if we put together four typical groups in ascending order of evolution, thus—

Water-plantain	3 sepals; 3 petals; 6 stamens (=twice 3); many carpels.
Damasonium	3 sepals; 3 petals; 6 stamens; 6 carpels (=twice 3).
Lily (tulip)	3 sepals; 3 petals; 6 stamens; 3 carpels (united).
Iris	3 sepals; 3 petals; 3 stamens; 3 carpels (united).

Thus it is clear that Damasonium has taken a step forward towards that reduction of parts which is so conspicuous a feature in the higher lily-like plants, and which, as we shall see hereafter, reaches a climax in those marvellous and highly-developed flowers, the orchids.

Again, in the water-plantain, we saw that each carpel contained only a single seed; but in Damasonium each carpel generally contains two. In fact, while in the true Alismas the carpels are many, small, and one-seeded, in Damasonium they are few, large, and two-seeded. This variation is a common mark of advance in the earlier stages of floral evolution; the carpels tend to grow fewer, bigger, and many-seeded. For example, in the buttercups, as in Alisma, they are numerous, small, and one-seeded; in the marsh-marigold they are reduced to five or ten (one or two whorls of five each), much larger and longer, and containing several seeds apiece.

Now, what is the reason of this advance? Clearly, plants must derive some advantage from the change, or it would not constantly occur as a concomitant of higher development in various families. A little reflection will serve to show us what that advantage really is. In the primitive plants with one-seeded carpels, each seed has to be separately fertilised by a distinct act of impregnation; for every stigma on to which an insect brushes pollen only one seed in the end gets set. This, of course, necessitates the production of a large number of carpels, and compels the plant to ensure, as far as possible, the separate impregnation of every one among them. Hence it becomes an advantage for the plant to produce fewer and larger carpels, each containing two or more ovules; because, in that case, each single act of impregnation suffices to fertilise two or more seeds. Accordingly, we find that as plants rise in the scale of evolution, they usually at first lessen the number of their separate carpels, while increasing the number of seeds in each. Later on, as we shall observe hereafter, the number of seeds also begins to decrease; but that is only when, by improved methods of fertilisation, protection, and dispersion, and increased richness of the seeds themselves, the plant is able to dispense with the necessity for producing an immense number of seeds from each flower. Efficiency, in such cases, serves the species in place of quantity.

Another interesting peculiarity of Damasonium consists in the fact that all six of its carpels are not quite free and separate from one another throughout all its life, as is the case with those of the Alismas and the buttercups: they are joined together at the bottom on the inner side to the stem or axis of the flower. This is the first beginning of that tendency which finally produces the compact, three-celled ovary of the lilies, where the three carpels have coalesced altogether, though their original distinctness is still marked by the walls of separation which divide the three cells from one another. Among the buttercup group, the monkshood is in this respect the exact analogue of Damasonium, for its three carpels are also usually joined together slightly at the base. If the lilies, however, had been developed from an ancestor at precisely the same stage as Damasonium, they would, of course, have had six cells to the ovary, instead of three, as is actually the case. The carpels of Damasonium taper to a sharp point, and are arranged radially like a conventional star, whence it gets its second name—*stellatum*, or starlike, and its other title of Actino-

Damasonium stellatum.

carpus, or "ray-fruit." I believe this arrangement of the carpels has reference chiefly to the fertilisation, so as to allow the insect's head to touch each stigma in succession easily and certainly; but it also doubtless aids in dispersing the seed equally in all directions.

It is very seldom that we can trace the marks of evolution continuously along a single line. We must rather pick out here and there separate indications of its general tendency, each surviving step or link being oftener a mere analogy (or independent similar development) than an actual survival of the various stages in the pedigree of the higher kinds. This is very strikingly seen in the Alisma-like plants, where several species preserve different levels of development, not in all parts alike, but one part in one species and one part in another. For example, take our two British arrowgrasses (*Triglochin maritimum* and *T. palustre*). In one respect, both these plants approach closer to the lily type than even Damasonium, for they have their carpels united during the flowering stage around the axis, though they separate from it into distinct pieces when ripe. Yet in another respect they are still as primitive as Alisma, because they contain only one seed in each carpel. Let us look for a moment at these two curious and common, but inconspicuous, little plants.

The arrowgrasses are degenerate small Alisma-like weeds, growing among tall marsh plants and grasses, and compelled by their habitat to decline into the practice of wind-fertilisation. Hence, like all other wind-fertilised plants, they have no bright-coloured petals. All six perianth pieces (that is to say, sepals and petals alike) are simply green, and they are very small or almost scale-like. Colour is not here needed to attract insects, and so the plant dispenses with it altogether. There are six stamens, hanging rather looser than in Alisma, so as to shed the pollen to the wind, though much confined by the scales of the perianth; and the carpels have each a feathery stigma, protruding from the flower, so as easily to catch the pollen dropped by the stamens. This feathery state of the stigma is very common among wind-fertilised plants, as it exactly suits their habit

Triglochin palustre.

of life. In fact, the arrowgrasses are Alismas which by degeneration have very nearly reached the same state as the true grasses; only, these latter are degenerate lilies, starting from a higher level in the evolutionary order. The head or spike of flowers in arrowgrass reminds one very closely of the common plantains (*Plantago*), which are degraded relatives of the veronicas, with closely similar habits.

Our two kinds of arrowgrass differ between themselves in the matter of carpels. The marine kind (*Triglochin maritimum*), which grows in salt marshes, is clearly the more primitive in type of the pair, for it has six carpels, like Damasonium, reduction having gone here only so far as to leave two whorls. In the flower there is thus a six-celled ovary, in the ripe fruit the cells divide as six distinct carpels. The fresh-water species (*T. palustre*), common everywhere on the border of streams, has only three, the reduction here having left but a single whorl. Thus this species, when in flower, closely resembles the true lilies, and is only distinguished from them when in fruit by the

fact that its carpels separate from one another as they ripen, so bearing witness to their primitive distinctness in the original ancestor. Still, a great many things conspire to show us that this Triglochin is not really a predecessor of the true lilies, but only an analogue, that is to say, a plant which has independently developed to some extent in the same direction. The Flowering Rush (*Butomus*), which we shall examine hereafter, though less like the lilies in its ovary, stands really nearer to them in genealogical order, as we shall see by-and-bye; but I must defer the consideration of that plant, as well as of the pretty arrowhead, to our next paper. We shall then have finished with the British representatives of these earliest Alisma-like monocotyledons.

AMONG THE INDIANS OF GUIANA.

(SECOND NOTICE.)

BY EDWARD CLODD.

NOT only do the physical characteristics and languages of the Indians of Guiana mark them as members of the Red Race, but their family system also, this being identical with the totem system of the North American Indians, which was explained in a recent number of this journal.* In the origin of the names of a number of families, of which Mr. Im Thurn gives a list, the belief in descent from the eponymous animal, bird, or plant is evidenced. Their marriage system is exogamous, *i.e.* the women are taken from other tribes, unions within the tribe being, except at missions, a rare event. The practice of reckoning descent through the mother prevails, and intermarriage with relations on her side is forbidden.

The ancient custom described by Herodotus, and familiar to us through the discoveries in the Swiss lakes and elsewhere, of living in houses raised upon piles, prevails among some of the Guiana Indians. Like the early Neolithic immigrants, they are not only hunters and fishers, but tillers of the ground, the harder fieldwork falling to women in the cultivation of the cassava plant, from the root of which their bread is made. The Indian has his spasms of work and long spells of rest, nor, where wants are simple and, on the whole, easily supplied, need he act otherwise.

> To be, contents his natural desire,
> He asks no angel's wing, no seraph's fire.

And in reading the account of his daily life one is inclined to agree with Professor Huxley that it is preferable to the squalid and sunless existence of the unpitied "wastrels" in the slums of great cities.

The curious and, to us, unmeaning custom of "la couvade" prevails in Guiana. When a birth happens, the father has to take to his hammock, and is tended just as if *he* had suffered the pains of child-birth. He has to abstain from food for a time, and for a still longer time from killing and eating large animals, lest harm come thereby to the child. The Rev. Mr. Brett, in his Travels in Guiana, describes how the "man in robust health and excellent condition, without a single ailment, was lying in his hammock in the most provoking manner, carefully and respectfully attended by women, while the mother of the new-born infant was cooking, none apparently regarding her." This odd custom probably has its origin in that barbaric feeling of a common sympathy between man and everything else which is the offspring of belief in their connection, and with which is therefore related the fear of harm through bad spirits indwelling in animals. Only such crude philosophy can seemingly explain an otherwise unintelligible practice which, if it were local, we should dismiss as eccentric, but which is traced from east to west, and the tradition of which, as Mr. Black points out in his work on "Folk Medicine,"* survives among the Basques, as also in France, and in Ireland. "The husband does not, indeed pretend to suffer the pains of labour, but the nurses boast that they possess the power of transferring the sufferings to him or to any other person they please. In earlier times she brought the pretended pains, for her appearance was tantamount to a declaration that his confinement and restricted living must commence. Now, the nurse threatens a real transfer, and not understanding why the husband should be the only sufferer, she boasts of being able to give the pains to any man, particularly, my informant says, to old bachelors."

The father is supreme in the family; but, in the settlement, authority rests with the man who has proved himself the ablest warrior or hunter, he, in his turn, standing in awe of the peaiman or medicine-man, minister to ills both of body and mind. Mr. Im Thurn gives an amusing account of the treatment to which he submitted himself at the hands of the peaiman when suffering from headache and fever. Space does not permit us to extract it, and it must suffice to say that one of the remedies consisted in indescribable yells and growls, which the peaiman kept up for six hours without ceasing! But the treatment, being external only, bears comparison with that adopted by a medicine-woman among the Piutes in the case of a patient suffering from an attack of grizzly bear, her compound consisting of mixed weeds, chewing-tobacco, the heads of four rattlesnakes, seasoned with red-pepper, and diluted with crude petroleum, one pint to be taken every half-hour. It is needless to add that, although a brave warrior and noted for fortitude under suffering, after taking the first dose, "the subsequent proceedings interested him no more."†

But the peaiman wars not so much against flesh and blood as against unseen principalities and powers, his chief function being as antidote to the kenaima, or slayer. The real kenaima is one who carries out the vendetta, or law of revenge; the imaginary and more dreaded kenaima is believed to exercise the power of separating his body and spirit, which latter, whether it enters into beast, bird, or insect, is still controlled by its owner, at whose door all diseases and death are laid. The peaiman's duties are, however, best understood in connection with the religion of the Guiana Indians. Concerning this, Mr. Im Thurn has gathered, without bias, valuable materials in support of the modern doctrine that "animism," or a belief in the existence of spirits as distinct, but not necessarily separate, from bodies, whether animate or inanimate, is the universal and earliest form of religion, and the germ from which the higher faiths of mankind have developed.

Here is a people, with civilisation on their seaboard, who have made scarce any advance beyond that primitive thought which draws little or no distinction between living and lifeless things, which credits not only swirling rapids and waving branches, but motionless rocks and stones, with indwelling spirits who can leave their abodes at will and pass into things intangible as the air or substantial as the mountain, on mission ever baneful. In the phenomena of dreams and visions, of sleep and death, we find here, as at like levels of culture, the sources of that belief in spirits anywhere and everywhere. Mr. Im Thurn relates how the vividness of their conviction of the actual occur-

* See KNOWLEDGE, Sept. 7, 1883, pp. 146-7.

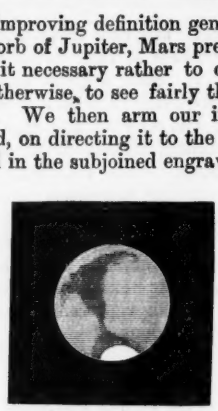
* Page 179. (Published by the Folk-Lore Society.)
† *Vide* "Dorman's Primitive Superstitions," p. 359.

rence of the things dreamed caused the Indians of his party to come perilously near doing mischief to their supposed foes. And as with the dreams, so with the apparitions which appear to them in waking moments under abnormal states of the body. The identity between man and brute, which is established on these lines, is not so easy to follow in the case of inanimate objects which are without motion, but the barbaric mind draws no fine distinctions. The injuries caused by falling over rocks, or *vice versâ*, are referred to the spite and evil designs of spirits dwelling in them, and awe of them is quickened if the rock or erratic boulder bears any mysterious carvings or wears the shape of man or beast or monster. Beyond this general animism the Guiana Indians do not appear to have risen; the after-existence of the spirit is taken for granted, but only whilst the memory of the dead survives can their continued existence be assumed. The brain which gets puzzled at any attempt to count beyond the fingers can hardly frame conceptions of endless duration, or of any everlasting abode, whether of happiness or misery. The heaven of the Indian is but another country in some other part of the world, beyond the sea which he cannot cross, but whence the white man comes; or above the solid blue which he cannot pierce, whence descended the ancestors, and whither go the birds and the spirits of the wonder-working peaiman. In that spirit-realm no member differs from another in glory—they meet in "equal sky," as Pope describes it. Hence we find no approach towards polytheism, even at its lowest; still less towards any alternating supremacy of gods, or one almighty deity. His attitude towards spirits is to keep on guard, or call in the peaiman, against such as may harm him; as to the rest, since they are on his side, he need not trouble about them. He prays to no being whatsoever; rites and ceremonies, full moons and sabbaths exercise him not; he is only careful to abstain from meats forbidden or untested, lest evil befall him and his thereby. In brief, animism, as it exists amongst the Indians of Guiana is, as Mr. Im Thurn remarks, "much more primitive than has yet been suspected by most students of religious evolution." In the final chapters an account is given of the folk-lore and culture-myths of the tribes; and of the various antiquities, amongst these the pictured rocks of unknown origin, and the shell-mounds corresponding to the "kitchen middens" of the Baltic. But enough has been said about this charming book to indicate what a banquet the author has spread for the enjoyment both of the naturalist and the student of culture. They will find in the *menus* which the classified indices furnish, ample details of the good cheer, and in the illustrations useful ornaments to the text.

MARS IN A THREE-INCH TELESCOPE.

By a Fellow of the Royal Astronomical Society.

THE study of Areography (Greek Ἄρης Mars), or minute Martial detail, can only be properly carried on by the aid of a telescope of considerable size and power. Notwithstanding this, we shall find that a good deal that is curious and instructive in connection with the physical structure of this planet is well within the capabilities of the instrument we are using; and we propose to-night to examine such features of his surface as are susceptible of exhibition with only three inches of aperture. A haze of cirro-strati is drifting over the sky as we turn our telescope on to Mars; but, as we observed on page 101, in connection with Jupiter, this really has the effect of subduing the general glare of the planet, sharpening such detail as is perceptible, and improving definition generally. Compared with the mighty orb of Jupiter, Mars presents such a small disc that we find it necessary rather to overpress magnifying power than otherwise, to see fairly the principal markings he exhibits. We then arm our instrument with a power of 204, and, on directing it to the planet, behold the spectacle depicted in the subjoined engraving.



Mars, Feb. 18, 1884, 9h. 35m. G.M.T., Power 204.

Unlike Jupiter and Saturn, the disc of Mars—in its present phase—appears circular. By-and-bye, at the beginning of May, when Mars is in so-called "Quadrature" with the sun, he will be very perceptibly gibbous (Fig. I., p. 86), but under no circumstances has any trustworthy measurement shown a sensible excess of his equatorial over his polar diameter. Dawes found the ellipticity absolutely imperceptible. Regarding then this circular disc, what do we see? A bright white patch at the bottom—or north pole—of the planet is probably the first thing that will arrest the attention of the observer, contrasting as it does markedly with the general orange tint of the planet. Bounded on one side by the limb, this brilliant marking exhibits the form of a double convex lens. It is entirely surrounded on its southern, or upper edge, by a greenish grey dark marking, from which rises another of a shape akin to that of the old-fashioned champagne-glass of the days of our fathers and grandfathers. Instead, however, of terminating in a symmetrical rim, the southern extremity of this bifurcates in a fashion which somewhat suggests the spreading of the wings of a sea-gull. The central southern portion of this is the darkest part of it. Our sketch was made, as stated above, at 9h. 35m. at night. Had we deferred it until four o'clock the next morning, the planet would have presented a very different aspect indeed; for Mars rotates on his axis in 24h. 37m. 22·735s. (thus taking a little more than half an hour longer than the earth to complete one rotation), and from this cause, of course, fresh portions of his surface are presented to our view, as the night advances, just as in the case of Jupiter. There is, however, this very notable difference between the markings on the two planets, that whereas, as stated on p. 101, the detail on Jupiter is in no legitimate sense permanent, existing as it probably does in a vaporous and very mobile envelope of enormous extent, in Mars the markings are persistent, and certainly form parts of his actual solid (and liquid) surface; so that maps of no inconsiderable accuracy have been formed of them. What, then, do they signify? It seems in a high degree probable that, in looking at Mars, we are regarding a miniature of our own world. That the general surface of the planet may well represent land of a geological structure allied to the "Triassic," or New Red Sandstone Rocks so well displayed at Exmouth, Dawlish, and Teignmouth in this country; that the darker markings are nothing but oceans, seas, straits, and lakes; while the conspicuous white patch at the pole has its origin in the existence there of the huge tracts of glacier and snow-covered land and ice-locked sea of the Martial Arctic regions. For, during the present

opposition of Mars, the north pole of the planet is turned towards the earth (Fig. 5, p. 72), and it will be seen, from the position of the planet's axis with reference to us, that his south pole is wholly hidden, from being on the other side of him. The reason for this is worthy of a brief examination. Mars goes round the sun in 686·98 days; the earth describing her orbit in 365·26 days; so that being in opposition, at any given date, an interval of 779·84 days must elapse ere they return to it. At least, this is the mean period between two successive oppositions; but owing to the great eccentricity of Mars' orbit, this varies as oppositions occur near his perihelion or aphelion (points of his nearest approach to and greatest recession from the sun) respectively. Moreover, the equator of Mars is inclined some 27° to the plane of his orbit. As the inclination of our own equator is only 23½° to the ecliptic, it is evident that the vicissitudes of the Martial seasons must be more aggravated than in our own case. But we have spoken of this tilt of the planet's axis mainly for the purpose of pointing out that while at certain oppositions, the north pole of the planet must be turned towards us, at others we must see his south pole; while at intermediate points—answering to our terrestrial equinoxes—we must see both poles, just as in the circular maps of the earth which form the frontispieces to so many books on geography. The south pole of the planet, as we have said above, is wholly invisible now; but, during the memorable opposition of 1877, a great white lenticular-shaped patch on the southern limb of Mars formed just as conspicuous a feature as the corresponding north polar one (then invisible) does now. From all this it will be evident that to get a true idea of areographical detail, we must watch, and carefully draw, Mars during several oppositions. It is very remarkable to note what a curious change in the aspect of any given feature, far north or south of the equator of Mars, is produced by foreshortening. Nothing but long-continued observation and abiding faith in the irrefragable principles of perspective will enable the student to identify a given spot or marking, when viewed under so very different an aspect. The best popular map extant of the leading Martial details is the one given by Prebendary Webb on p. 146 of his altogether admirable book, "Celestial Objects for Common Telescopes." It is, however, drawn on Mercator's projection, with its grossly-exaggerated polar dimensions; and the young observer must make allowance for this in using it for regions removed by any considerable distance from the equator. The great inverted conical marking shown in our sketch above is chiefly composed of the Kaiser Sea. The thin arm stretching out to the left is a part of Flammarion Sea; that to the right a portion of the Dawes' Ocean. The Delambre Sea (confused by foreshortening with Nasmyth inlet to the right), forms the dark marking surrounding the snow cap at the north pole (bottom) of the planet. Situated some 150° in longitude from the Kaiser Sea is a very notable marking in the southern hemisphere of Mars—unfortunately in an unfavourable position for the observer just now—which is called Terby Sea, and which, surrounded by Kepler land, presents a somewhat ludicrous resemblance to an eye. In the absence, however, of a map, detailed descriptions of particular markings become merely nugatory.

It only remains in conclusion to mention a few facts in connection with the general aspect of the planet. We have spoken of the very conspicuous white patches at, or near, the poles of Mars; patches occasionally so brilliant that irradiation causes them to appear as positively projecting slightly beyond the outline of his limb. That these, as we have previously intimated, consist of ice, or snow, or both, the evidence afforded by the spectroscope seems to render practically certain, showing, as it does, the presence of large quantities of aqueous vapour in the Martial atmosphere. There is, however, a somewhat similar appearance, which we have, among others, ourselves observed, that of lenticular-shaped white markings round the limb, which are by no means so easily explicable. A very little attention will show the observer with the telescope that the ruddy tint of the planet's face is most marked towards the centre of the disc, and that the limb is notably paler (occasionally so much so as to seem nearly white, as was the case in the autumn of 1879); but the markings of which we are now speaking are large white patches in the eastern and western limbs of Mars, for which it is anything but easy to account, though they are probably atmospheric. An extremely ingenious explanation of the general brightness of the limb will be found on page 65 of the "Essays on Astronomy," by the Editor of KNOWLEDGE. Mars has been described as a miniature of our own earth; and undoubtedly he does present features connecting his physical structure more closely with ours than any other planet; but still, irrespectively of size, there are differences which cannot fail to strike the thoughtful observer. The most salient of these is the difference of distribution of land and water. On the earth only about 51,500,000 square miles consist of dry land, while 145,500,000 square miles are covered by water. On Mars the land so far preponderates that the largest oceans, or rather seas, can only be described as more or less land-locked. Schiaparelli claims to have discovered a strange network of "canals," uniting various portions of the seas of the planet hitherto considered to be isolated; but, admitting, for argument's sake, the objective reality of these features, they are hopelessly beyond the optical power we are employing. We may mention, in conclusion, that a curious collateral indication of the existence of clouds, or vapour in some form, has been observed, in the shape of the dimming, or partial obscuration, of spots and markings on the surface of Mars; while others, at no great distance, have simultaneously retained all their usual sharpness and comparative precision of outline.

PLEASANT HOURS WITH THE MICROSCOPE.

BY HENRY J. SLACK, F.G.S., F.R.M.S.

IN the water mentioned in the last paper was a creature exhibiting to a wonderful extent the shape-changing power which those who like learned-looking words may call metabolism. Many infusoria besides the amœbæ can assume very different forms, and the common rotifer, which stands much above them in organisation, has an integument so elastic that it permits the creature to elongate itself like a worm, swell out like a pear, or become almost as round as a plum-pudding. The amœbæ have no permanent integument. Their outside readily becomes their inside, or *vice versâ*, but the layer which happens to be outermost and in contact with water appears less mobile than other layers which happen to be inside, and with their molecules surrounded on all sides by similar molecules. The little animal we have now before us has a very definite shape in its quiet state, and lives in a skin of considerable consistency and toughness. It is called *Astasia trichophora*. The first word indicates its belonging to a genus of form-changers, the *Astasiadæ*, and the second refers to its long whip, but not very accurately, as it is not at all hairlike. Fig. 1 is copied from Kent's valuable work representing

the little animal magnified 600 times linear. When quiet and stretched out it is an elongated somewhat pear-shaped bag, containing a multitude of granules, and having for its locomotive organ a very long whip, pretty thick at the base and thinning out gradually towards the tip, which is very fine. The letters *p h* indicate a mouth, and gullet, or pharynx, and *n* the nucleus and nucleolus, or, as Huxley calls these things, the *endoplast* and *endoplastule*. Balbiani thought these organs—which are found throughout the infusoria—represented male and female elements; but later observers differ from him, although it is admitted that they are

Fig. 1.

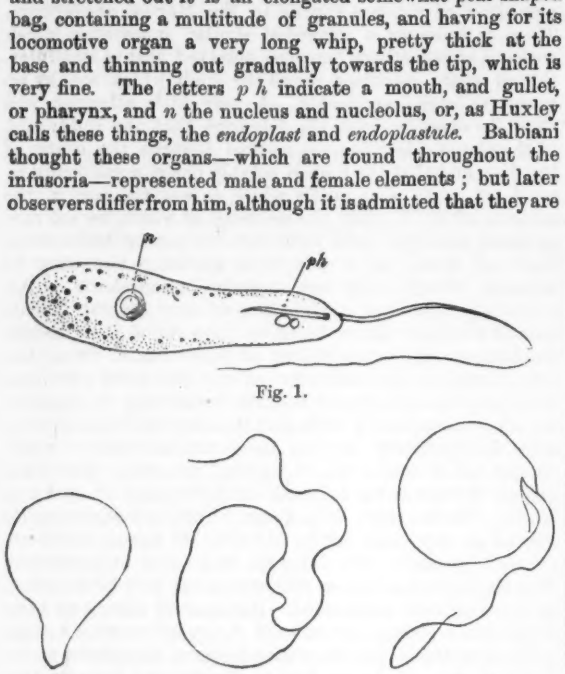


Fig. 2. Fig. 3. Fig. 4.

Fig. 5. Fig. 6.

Asbasia Trichophora—Fig. 1 from Kent's copy of Bütschli, × 600. Figs. 2–6 outlines of changing forms as seen by the writer.

concerned in generative processes. The Astasia family is in many respects like the Euglenæ, but without the rich colour and the eye spot common to the latter. Their sarcode, or flesh, if so we may term it, is colourless, and their mouths larger and more stretchable than the tiny orifices of the Euglenæ. Both families are wonderful form-changers, but the species before us is, perhaps, the most astonishing performer of this sort. Not for two seconds was the one under notice either quiet or of one shape, and this double activity of locomotion and figure-alteration went on for several hours, and might have lasted longer if the water-drop had not dried up. Figs. 2–6 show in outline some of the strangest forms it assumed. In No. 2 the whip is not seen. It was all curled up on the other side. This whip exhibits, very strikingly, a thoroughly vital character. Every bit of it is as alive as a monkey's tail, and in this it differs entirely from the cilia common to so many infusorians, and which behave like elastic rods, and are moved from their bases, as a switch may be moved by twists of the wrist. It would be quite impossible even for a Japanese juggler to imitate many of the movements of this whip by hand-play with an elastic rod. The swimming is dexterously performed all the while the form-changes are produced, and there is nothing like random or awkward bumping against other objects. With human skill it would be difficult enough to row and guide an object of fixed shape with one long whip; but to adjust its guiding action to the rapidly changing and bewildering shapes of this astasia would indeed be wonderful. Why does the creature do these things? and what stimulates it to makes such alterations of its shape? The locomotion of the infusoria answers a double purpose. It carries them about as hunters after food, and it brings them into contact with the water currents their motions occasion, and thus assists their respiration. The changes of shape may help respiration, digestion, and also the elimination of waste matter. The food particles must be tossed and shaken in the digestive fluids as effectively as in pestle-and-mortar manipulation, or brisk spoon-stirring to quicken solution, and as any of the contents get aërated, either through the integument or by inception of fresh water, they must carry their supply of oxygen to all parts of the creature. No structure can be discovered in the whip, but it appears a sensitive organ as well as a locomotive one.

Ehrenberg first described this little animal as *Trachelius trichophorus*, thus placing it in a higher rank than is now assigned to it. Kent says he mistook "the thick, cord-like flagellum for an elongate neck-like prolongation of the body, similar to that met with in the ciliated genera, *Lachrymaria*, *Amphileptus*, and *Trachelius*." This was a mistake easy to make with the imperfect object-glasses and means of illumination at the great naturalist's disposal, and it was one of those errors that point to a truth. A careful examination with an excellent oil immersion, $\frac{1}{12}$th of Leitz, shows that the whip is used very much like the long neck of a Trachelius or Amphileptus. The term *flagellum*, or *whip*, which applies very well to many lash-like swimming organs, is an insufficient one for the long instrument of this Astasia, which may be often seen exploring with its tip, and executing movements more like a proboscis than a whip.

Kent says, concerning the internal structure of this creature, that Bütschli finds its contractile vesicle of "a somewhat complex character, the water discharged in the act of systole being partly driven into smaller lateral diverticula, as obtains in *Entosiphon* and certain species of *Anisomena*, and as also observable in *Paramecium*, and among higher ciliata."

It multiplies by *long division*—or longitudinal fissure, and can also develop internal germs.

There are many so-called "species" of Astasia, but how many deserve that term is doubtful. The water our specimens were taken from abounded, in a minute form, like very small shape-changing bags filled with shining granules at the widest end. They altered their shape, however, within much narrower limits than the bigger fellow, who supplied amusement for a considerable part of one evening.

TRICYCLES IN 1884.

By John Browning,

Chairman of the London Tricycle Club.

THE STANLEY SHOW.

THE novelties in tricycles this season are so numerous that space could not be spared in Knowledge to give even a list of them. I shall mention, then, only those which appear to me to possess great merit. If I omit to

name some contrivances which are undoubtedly very novel, it must not be supposed that I have overlooked them. There are many novelties brought out every year that are worse than useless—they are mischievous or even dangerous. The "Rover," which I have already referred to in my previous articles, seemed generally to be appreciated as highly as I had anticipated and had led my readers to believe it would be.

The "Traveller," No. 2 (could not the makers call it the "Triple-Steering Traveller," the "Triplex," or the "Triplycycle," to distinguish it?) came in for a good share of notice, which it well deserves, for the oftener I ride it the easier it seems to *travel*, and the more perfectly it steers. There were many machines of the "Humber" type at the Floral-hall; but while many of them were bad copies of the original model, this had special advantages peculiar to itself; the height of the saddle being adjustable, as well as the steering-handles, and the loose backbone and free hind-wheel being replaced by a backbone and hind-wheel always under the control of the rider.

The principal feature of the Stanley Show, however, I consider to have been the numerous two-speed gearings.

Three special machines with such gearings, kindly made for me to my own specifications, were exhibited, each by a different maker — a special light "Europa" by the St. George's Foundry Company, a special light "Sterling" by Adam Burdess, and a special light front-steerer by Rucker.

Of the "Europa" and the "Sterling" I can report most favourably, so far as I have been able to try them. The "Rucker" was exhibited in an unfinished state, and it is not yet completed, Mr. Rucker, like a true artist, desiring to almost remake it, though I would have accepted it as it was. It may be at the Agricultural Hall on the 6th inst. I expect a great deal from this machine, and I am not alone in this anticipation, for my friend, Mr. Grace, one of the best judges of a tricycle known to me, selected it, without my naming it to him, as the most promising machine in the whole exhibition. This "Rucker" will combine, I hope, the lightness and speed of a "Humber" with the safety of a front-steerer and the comfort of a two-speed machine.

The Coventry Machinist Co. exhibited two new two-speed gearings. One which had two chains, one on each side of the machine, will, I think, prove valuable, and may prove to be the best yet introduced. It is a double driver with either chain, that is when either the slow-gearing for hill climbing or the high-speed arrangement is brought into action. I hope to be able to give some definite details and information respecting this machine shortly.

The same firm exhibited a novel arrangement of a tricycle on springs, which promises to reduce vibration when riding over rough roads to a minimum.

Two special features of the exhibition seemed to me to be a reduction in the size of the wheels and a reduction in the weight of the machines.

A maker told me that a year ago only the demand for sociable tricycles was for wheels to be 48 in. in diameter, while now the principal demand is for wheels 44 in. only.

The makers of the "Rover" turn out a machine with 42-in. wheels, but destroy half its value by refusing to supply it except with level gearing. This 42-in. machine, geared up to about 50 in., would be one of the best possible mounts for a strong rider.

Referring to light machines, Starley Bros. exhibited a new light roadster, weighing 67 lb.—a distinct advance on anything they have hitherto done, the style and finish being equal to the plan of the machine.

Dr. Richardson has stated that it is of the utmost importance that everything possible should be done to reduce the vibration in tricycles. Messrs. Lamplugh & Brown have applied an adjustment to their well-known long-distance saddles, which enables any rider to loosen the stretched leather and enjoy the luxury of riding with a sagged saddle; and whenever, from the effects of wet or hard riding, the leather gives too much, so as to touch the steel frame, by a simple turn or two of a nut, to restore the saddle to its former condition.

Messrs. Brookes have in their new lever tension saddle, in which the leather is attached to helical springs, given us a still more luxurious seat. The test of time alone will show whether the saddle will retain these advantages unimpaired; but the work is excellent, and it should be durable.

Many of the exhibitors at the Floral Hall were sadly cramped for space. There are only two buildings in the Metropolis where such a number of novelties in tricycles as our makers now annually produce could be adequately exhibited and easily inspected. These are the Agricultural Hall and the Crystal Palace. But, possibly before next February, the Battersea Park Exhibition may be added to the number. A great exhibition of tricycles will open at the Agricultural Hall, on Thursday, March 6, the day before this number of KNOWLEDGE is published. Many new inventions will be shown at this exhibition which were not ready in time for the Floral Hall. There will be nearly a thousand machines well displayed, and I hope to give an account of the principal novelties in early numbers of KNOWLEDGE.

THE UNIVERSE OF SUNS.

By RICHARD A. PROCTOR.

(*Continued from page* 97.)

BEFORE passing from the celebrated Memoir of 1785, —which I have been careful to discuss at some length on account of the imperfect nature of the description usually given,—I deem it well to quote yet one more passage, partly because it contains sentences calculated to remove all doubt as to the nature of Herschel's ideas in 1785 respecting the galaxy, but chiefly because the closing words show that he was prepared for the possibility that later researches might lead to modifications of his theory:—"The rich parts of the Milky Way," he says, "as well as those in the distant broad part of the stratum, consist of a mixture of stars of all possible sizes, that are seemingly placed without any apparent order. Perhaps we might recollect that a greater condensation towards the centre of the system than towards the borders of it should be taken into consideration; but with a nebula of the third form, containing such various and extensive combinations as I have found to take place in ours, this circumstance, which in one of the first forms would be of considerable moment, may, I think, be safely neglected. However, I would not be understood to lay a greater stress on these calculations than the principles on which they are founded will permit; and if hereafter we shall find reason, from experience and observation, to believe that there are parts of our system where the stars are not scattered in the manner here supposed, we ought then to make proper exceptions."

In the year 1789, Herschel prefaced a list of 1,000 nebulæ by an essay of considerable interest, relating to the formation of nebulæ. His reasoning does not readily admit of condensation; and to give it at full length would occupy more space than can here be spared. I would, however, advise the student of Herschel's theories to pay close attention to this paper, as it contains a very distinct description of the ideas entertained by Herschel at this comparatively

early stage of his progress as an observer of the sidereal heavens. He enumerates at the close the theory that nebulæ are stellar systems in various stages of development, the planetary nebulæ in which the (supposed) component stars seem most equally distributed, being the most aged and even approaching towards a period of change or dissolution. "This method of viewing the heavens," he remarks, "seems to throw them into a new kind of light. They now are seen to resemble a luxuriant garden, which contains the greatest variety of productions in different flowering beds; and one advantage we may at least reap from it is that we can, as it were extend the range of our experience to an immense duration. For to continue the simile I have borrowed from the vegetable kingdom, is it not almost the same thing whether we live to witness successively the germination, blooming, foliage, fecundity, fading, withering, and corruption of a plant, or whether a vast number of specimens, selected from every stage through which the whole plant passes in the course of its existence be brought at once to our view?"

But the time was now approaching when Herschel was to abandon almost wholly the theory which he had enunciated in 1785, not only in its relation to the sidereal system but to nebulæ and star-clusters. Already in 1796, he pointed out that the hypothesis of a general uniformity of structure in the galaxy is too far removed from the truth to be depended upon. This, indeed, does not imply a distinct withdrawal from the theory of 1785, which was not in reality based on a general uniformity of stellar distribution; but the stress now placed by Herschel on probable varieties of structure is a novel feature in his theoretical treatment of the subject. I pass on to the memoir of 1802, which is the first in which Herschel distinctly states that his ideas had undergone a change.

It was in this paper that Herschel enunciated his views respecting binary stars. Such stars, as well as all forms of multiple stars are now for the first time distinguished from insulated stars. But it is specially worthy of notice, here, that Sir W. Herschel speaks of the insulated stars as alone, in all probability, the centres of planetary systems resembling our own. "The question will now arise," he says, "whether every insulated star be a sun like ours, attended with planets, satellites, and numerous comets? And here, as nothing appears against the supposition, we may from analogy admit the probability of it. But, were we to extend this argument to other sidereal constructions, or still farther, to every star of the heavens, as has been done frequently, I should not only hesitate, but even think that from what will be said of stars which enter into complicated sidereal systems, the contrary is far more likely to be the case; and that probably we can only look for solar systems among insulated stars."*

Now it will be obvious from a consideration of Herschel's theory of 1785, that though he by no means regarded the stellar system as spread with that general uniformity of distribution described in the popular travesties of the theory, yet he was equally far from regarding the stars of the Milky Way—taken in a body—as differing from the stars forming our constellations. The very principle on which his star-gauges were based requires that any considerable portion of the Milky Way should resemble in general respects a corresponding portion from regions of the stellar system nearer to the earth. There would be irregularities,—large stars and small stars, aggregations and segregations,—in one portion as in the other, and though the details of one would not resemble those of the other, yet a general resemblance Herschel undoubtedly supposed to exist between all such portions of the sidereal system. Now, however, in 1802, he entertains a widely-different view. "The stars we consider as insulated," he writes, "are also surrounded by a magnificent collection of innumerable stars, called the Milky Way, which must occasion a very powerful balance of opposite attractions, to hold the intermediate stars in a state of rest. For though our sun, and all the stars we see, may truly be said to be in the plane of the Milky Way, yet *I am now convinced by a long inspection and continued examination of it, that the Milky Way itself consists of stars very differently scattered from those which are immediately about us.*"*

And when, having described the various orders of star systems he passes on to the consideration of the Milky

Fig. 6.—The Clustering Region of the Milky Way in Cygnus.

Way, Herschel shows clearly in what this difference of construction consists, according to his new views. "On a very slight examination," he says, "it will appear that this immense starry aggregation is by no means uniform. The stars of which it is composed are very unequally scattered, and show evident marks of clustering together into many separate allotments. By referring to some one of these clustering collections in the heavens, what will be said of them will be much better understood than if we were to

* Sir W. Herschel mentions among the stars which he supposes to be far removed from the disturbing influence of other orbs—Arcturus, Capella, Vega, Sirius, Canopus, Markab, Bellatrix, Menkar, Schedir, Algorab, and Propus. I cannot see why Propus—a fifth magnitude star on the borders of the Milky Way should be included in the list. All the others are bright stars more or less isolated, even Algorab being of third magnitude as well as in an exceptionally barren region. Propus was probably a misprint for Procyon.

* As some writers who have either not studied Sir William Herschel's original papers at all, or have paid but superficial attention to them, have bluntly contradicted my statement (which Struve also made) that he gave up later in his career the theory which he had adopted in 1785, and the principle of generally uniform distribution on which it was based, I have emphasised the closing words of the above passage. Writing seventeen years after the theory of 1785 had been advanced (even then only tentatively) and sixteen years before the close of his wonderful series of researches the statement is unmistakably clear in purport.

treat of them in a general way. Let us take the space between β and γ Cygni for an example, in which the stars are clustering with a kind of division between them" (Fig. 6), "so that we may suppose them to be clustering towards two different regions. By a computation founded on observations which ascertain the number of stars in different fields of view, it appears that our space between β and γ, taking an average breadth of about five degrees of it contains more than 331,000 stars; and admitting them to be clustering two different ways we have 165,000 stars for each clustering collection. Now *the above-mentioned milky appearances deserve the name of clustering collections, as they are certainly much brighter about the middle and fainter near their undefined borders. For in my sweeps of the heavens it has been fully ascertained that the brightness of the Milky Way arises only from stars; and that their compression increases in proportion to the brightness of the Milky Way.*"

Let the reader note the reasoning here very carefully; for the passage is most important and Herschel's condensed style and the absence of any direct reference to the theory he had enunciated seventeen years before has caused the

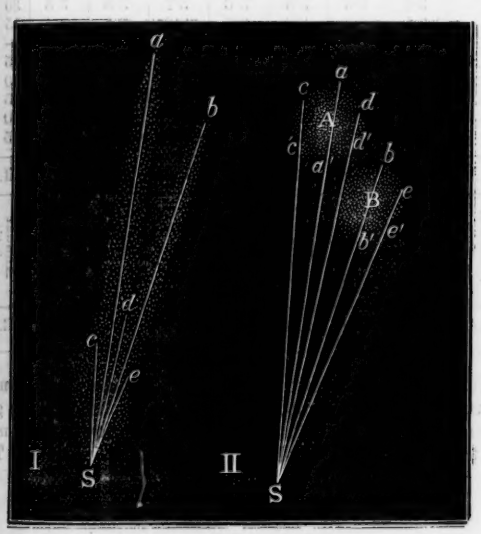


Fig. 7. Illustrating the views of Herschel (I.) in 1785 and (II.) in 1802.

significance of the passage to be commonly overlooked. Observe,—Herschel now regards this important portion of the Milky Way (the brightest part in the northern hemisphere), as composed of two clustering collections—that is, as surrounded on all sides by comparatively vacant spaces. Between us and this region of the Milky Way there lies, then, a vast space not so richly filled with stars. Now in the theory of 1785 Herschel explained rich regions of the Milky Way, such as those between β and γ Cygni, by supposing a great extension from the sun at S (Fig. 7, I.) towards the heart of each rich region as along the lines S*a* and S*b*, with relatively small extensions in directions S*c*, S*d*, S*e*. It involved a perfect change in the very conception of the Milky Way (as a whole) to explain the richness of these regions (typical, be it remembered, of many such regions) as due to a real aggregation of stars as at A and B (Fig. 7, II.) with such varying ranges of stars along the lines S*a*, S*b*, S*c*, S*d*, and S*e* as are shown in this second figure. The gauges can no longer be understood to indicate the extension of the sidereal system from the sun.

Sir William Herschel here abandons in a special case the very principle on which he had based the interpretation of the gauges. He had already in the same paper, as we have seen, abandoned the same principle as regards the Milky Way generally. Yet although he thus definitely gave up that principle generally, and specifically indicated by a remarkable illustrative case how and where he had abandoned it, that principle and the theory based (tentatively) on its application are calmly quoted in works on astronomy as if Sir William Herschel had definitely said the very reverse—or as if he had spoken thus:—"In 1784 I provisionally adopted a principle, and in 1785 I indicated a theory, which seemed suggested by the first application of that principle to the Milky Way. *Now after a long inspection and continued examination I am convinced the theory and principle are both sound, and that the Milky Way consists of stars scattered like those around us.*" Compare this with what he actually said (see the italicised passages above), and the absurdity becomes manifest.

We can understand, then, so much the more clearly what Herschel means when in this paper of 1802 he says that he *now* finds the Milky Way to consist of stars very differently arranged from those around us. But he makes further remarks respecting the clustering aggregations in Cygnus, which show yet more clearly what his new views were, though by a strange misconception those additional remarks have been supposed (even by the few who recognise his change of view) to signify that he regarded the question between the two views as an open one. He proceeds, "we may, indeed, partly ascribe the increase both of brightness and of apparent compression—in these clustering regions—to a greater depth of the space which contains these stars; but this will equally tend to show their clustering condition; for, since the increase of brightness is gradual, the space containing the clustering stars must tend to a spherical form, if the gradual increase of brightness is to be explained by the situation of the stars." Here clearly what Herschel means is, that we must take into account not only the clustering condition of two such groups as are shown at A and B (Fig. 7, II.) but also the fact that a line from the sun at S through the heart of each cluster (as S*a* through the cluster A and S*b* through the cluster B) passes through a greater range of stars than a line nearly towards the edge of either cluster as S*c*, S*d*, S*e*. (We see that *aa'* and *bb'* are greater than *cc'*, *dd'*, and *ee'*.) The gradual increase of brightness towards the middle, implies, he asserts, as strongly as the richness of the whole of these regions, that they are due to real clusters somewhat round in shape. He as explicitly accepts this view, in fact, as he implicitly rejects the view that the increase of brightness towards the centre is due to the state of things illustrated in the first drawing (I.) of Fig. 7. For in the latter case the increase would neither be gradual nor regular, unless we assumed a highly improbable adjustment of the two extensions in directions tending *exactly* from S.

In the paper of 1802, Herschel also for the first time advocates the theory that some of the nebulæ are not clusters of stars, but formed of some substance possessing "the quality of self-luminous milky nebulosity," and "possibly at no great distance from us." It is interesting to notice that he now no longer regards the light of the great nebula in Orion as arising from immensely distant regions of fixed stars. "Even Huyghens, the discoverer of it, was already of opinion that in viewing it, we saw, as it were, through an opening into a region of light. Much more would he be convinced now, when changes in its shape and lustre have been seen, that its light is not, like that of the Milky Way, composed of stars. To attempt even a guess at what this light may be would be pre-

sumptuous. If it should be surmised, for instance, that this nebulosity is of the nature of the zodiacal light, we should then be obliged to admit the existence of an effect without its cause,"—meaning probably that the zodiacal light is due to solar illumination, and that there is no sun which can be supposed adequate to the illumination of a region so large as that occupied by the Orion nebula. "An idea of its phosphorical condition," Herschel adds, "is not more philosophical, unless we could show from what source of phosphorical matter such immeasurable tracts of luminous phenomena could draw their existence and permanency; for, though minute changes have been observed, yet a general resemblance, allowing for the difference of telescopes, is still to be perceived in the great nebulosity of Orion, even since the time of its first discovery."

(To be continued.)

HOW TO MAKE USEFUL STAR MAPS.

By Richard A. Proctor.

IN Fig. 6 I have put in the stars so as to correspond to the year 1840. Otherwise the comparison between the two figures would have been interfered with. But it is very necessary, indeed, that in constructing maps from a catalogue or atlas of an old date, the student should pay attention to the effects of precession, by which all the stars are shifting their place, not indeed on the heavens, but athwart the meridians and parallels of our maps. The reason why this is important is very obvious. If we want to find where some object we wish to observe is situated, we mark it down according to its recorded place on our map. Now if this is done on a map constructed nearly for the same date as that corresponding to the place given to the object, all is well. But if, instead of this, the stars of the map are marked in for another date, our object will be wrongly placed among them. We must, in fact, shift the object, so as to correspond to the date of the map. This is very readily done if we know the rate at which precession is shifting the places of stars in different parts of the heavens.

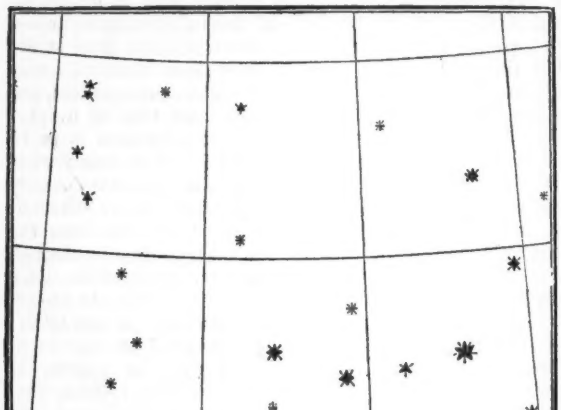


Fig. 6.

For this purpose the following table, abridged from one in my "Handbook of the Stars" (and calculated expressly for that work, which is now out of print), will be found useful:—

TABLE OF CHANGES DUE TO PRECESSION.

R.A.	0h.	1h.	2h.	3h.	4h.	5h.	6h.	R.A.
Annual variation in Dec.	20″ N S	19″ N S	17″ N S	14″ N S	10″ N S	5″ N S	0″	Annual variation in Dec.
R.A.	12h.	11h.	10h.	9h.	8h.	7h.	6h.	R.A.

Annual Variations in R.A.

Dec.	s.	s.	s.	s.	s.	s.	s.	Dec.
75° N	+3·1	+4·4	+5·5	+6·6	+7·4	+7·9	+8·1	75° S
60	3·1	3·7	4·2	4·7	5·1	5·3	5·4	60
45	3·1	3·4	3·7	4·0	4·2	4·4	4·4	45
30	3·1	3·3	3·5	3·6	3·7	3·8	3·8	30
15	3·1	3·2	3·2	3·3	3·4	3·4	3·4	15
—	3·1	3·1	3·1	3·1	3·1	3·1	3·1	—
15 S	3·1	3·0	2·9	2·8	2·8	2·7	2·7	15 N
30	3·1	2·9	2·7	2·5	2·4	2·3	2·3	30
45	3·1	2·7	2·4	2·1	1·9	1·8	1·7	45
60	3·1	2·5	1·9	1·4	1·1	1·8	0·8	60
75	+3·1	+1·8	+0·6	−0·4	−1·2	−1·7	−1·9	75
R.A.	12h.	13h.	14h.	15h.	16h.	17h.	18h.	R.A.
Annual variation in Dec.	20″ S N	19″ S N	17″ S N	14″ S N	10″ S N	5″ S N	0″	Annual variation in Dec.
R.A.	24h.	23h.	22h.	21h.	20h.	19h.	18h.	R.A.

In this table, the two upper rows of hours of R.A. must be taken with the right-hand table of declinations to give the annual change in R.A., while the two lower rows must be taken with the left-hand table of declinations. Two examples will serve fully to illustrate the use and meaning of this table.

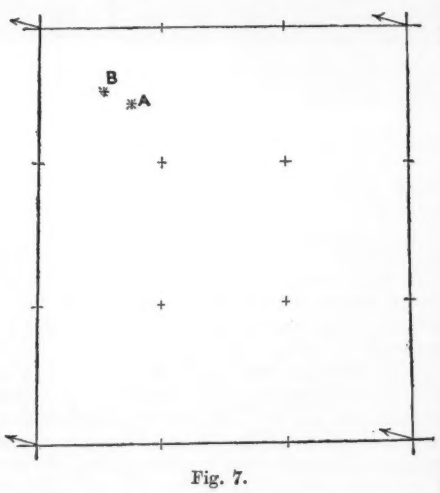


Fig. 7.

Supposing we are drawing a map which includes the

space between 3h. and 4h. of right ascension, and between 15° and 30° of north declination :—Then at the corner of this space, corresponding to 3h. R.A. and 30° N. Dec. (the upper right-hand corner), the change for one year will be 14″ in declination northwardly and + 3·6s. in right ascension. So for the upper left-hand corner of the space the change is 10″ in dec. northwardly, and 3·7s. in right ascension. At the lower left-hand corner the change in declination is 10″, and in right ascension 3·4s. ; while at the lower right-hand corner the change in declination is 14″, and in R.A. is 3·3s. Now we can draw at each corner of the space a little arrow indicating the change for as many years as the date differs from that of our atlas or catalogue, and each star as it is marked in must be shifted over a space equal in direction and distance to the arrow which falls nearest to it. Fig. 7 shows these arrows for such a space of the globe as we have considered above, and corresponding to the long interval of 100 years (that is, the variations 14″, 10″, 3·6s., &c., have all been multiplied by 100). A shows the place of a star in a map 100 years old, while B is its proper place for the present date.

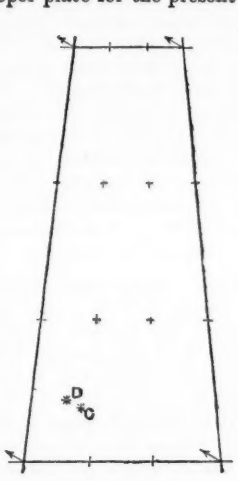


Fig. 8.

Secondly, suppose our space lies between 22h. and 23h. R.A. and 45° and 60° north declination. Then the table gives us the following changes :—

For upper left-hand corner 19″ N. in Dec. and + 2·5 in R.A.

For lower left-hand corner 19″ N. in Dec. and + 2·7 in R.A.

For upper right-hand corner 17″ N. in Dec. and + 1·9 in R.A.

For lower right-hand corner 17″ N. in Dec. and + 2·4 in R.A.

Fig. 8 represents the changes in this case, C being a star's place in a map a hundred years old as before, D its correct position.

The reader will find no difficulty in seeing how the table is used in all such cases, remembering the direction already given as to the use of the side columns with the upper and lower rows of R.A. ; and also that where the letters $\frac{N}{S}$ or $\frac{S}{N}$ occur the one nearest to the hour row we are using must be taken.

It need hardly be mentioned that as Figs. 7 and 8, are only meant to explain the use of the table, it has not been thought necessary to pay particular attention to the shape of the spaces.

THE *EDINBURGH* REVIEW OF SPENCER'S "FIRST PRINCIPLES."

To the Editor of KNOWLEDGE.

NOW that I have returned to my books I see clearly how that mistake of omitting exactly one line in the quotation from Mr. Spencer's "First Principles" came, viz., from the four words, "produces in us the," being repeated over each other in two successive lines, and my eye in copying mistook the second for the first. I wonder you did not perceive that with the book before you. And it was not the kind of mistake to catch one's attention in reading a proof of what I had not written—or rather, composed—myself. Mistakes of that kind, and worse, in copying have caused all the disputes about the various early MSS. of the Greek Testament, though of course the scribes thought their subject of infinitely more consequence than I did mine.

I did not like to contradict your version and derivation of *axiom* from ἄξιος without referring to dictionaries, both Greek and English. You say it is something "established by experience and worthy to be accepted." I find, as I expected, that it is exactly the contrary of the first of these things ; and the second applies to all truths. Axiom is a technical word, a mere English writing of ἀξίωμα, which in philosophy meant "that which is assumed as a basis of demonstration, or a self-evident proposition" ; for both which meanings Liddell and Scott give Aristotle as the authority. And I suppose they might have added Euclid, though I have not a Greek Euclid to refer to. I am satisfied Newton meant the same by his "Axioms or Laws of Motion," though he illustrated them by some experience which alone was quite insufficient to prove them. The first really involves the "principles of sufficient reason" too. Johnson defines an axiom as "that which is self-evident and cannot be made plainer by demonstration." That is the way I used it throughout the article, and I have no more to say of Mr. Spencer's use of it and "Postulate" than I have said there. I see, too, that Mr. Romanes in his last book on "Instinct" speaks of Mr. Spencer's habit of making definitions to suit his own purposes quite as strongly as I have.

THE EDINBURGH REVIEWER.

[Recognising the mistake in copying as certainly not intended I was not on the look-out for its cause, which doubtless was as the Edinburgh Reviewer thinks.

The word ἀξίωμα is as unquestionably derived from ἄξιος as στερεώμα from στερεος,—and it can no more be doubted means a something weighed (ἄγω) and not found wanting, than that the second means something made firm (στερεόω). If we are to argue about words, I might ask how "that which is assumed as a basis of demonstration" comes to be "exactly the contrary of that which is established by experience." To assume as a basis of demonstration what was not only *not* established by experience but directly contrary thereto would be to lay the foundation of a rather singular process of reasoning. In philosophy the word "axiom" was early used to signify a self-evident proposition, though earlier the word meant a "decision," "something thought worthy of universal acceptance," because "so established by experience as to admit of no reasonable doubt." Whoever gave the definition, "that which is assumed" &c. was unable to express in English what very likely he understood very well in the Greek ; for an "assumption" and a "decision" are two very different things, and the word "axiom" in each of its two uses,—the primary meaning, according to which experience was in question, and the

derived meaning, according to which obviousness was chiefly dwelt upon,—has always meant a "decision," not an "assumption."

The so-called axioms of Euclid are of both sorts, and of yet a third kind (8 and 9, Book I.),—they include (i.) decisions based on experience, (ii.) self-evident propositions, and (iii.) truisms (statements, indeed, which, in one aspect, may be regarded as merely definitions). Euclid however did not use the word *ἀξίωμα*, but called these preliminary statements *κοιναὶ ἔννοιαι*, common thoughts.

But the point is not what Euclid would perhaps have meant if he had used the word, nor even what Newton may have meant,—but what the Laws of Motion are. If I asserted that Newton used the word axiom in its proper sense as a decision based on experience, and deserving to be accepted, it was,—First because he is so careful to indicate a part of the experience by which the laws have been established, and secondly because he knew and shows over and over again that he knew, how manifestly incorrect it would be to describe as "self-evident propositions" principles which had escaped the keen insight of Greek philosophy, and had remained undetected during centuries of research and inquiry. The Edinburgh Reviewer says Newton perceived the first law to be a necessary truth on the "principle of sufficient reason," and again "it required Newton's power of divination to perceive why the first law is a necessary truth." If all the time and thought given to this subject before Newton failed to establish the law, and only the powers of divination possessed by him of whom it was justly said that he surpassed the whole human race in mental insight, finally established the law, then words must change a little in meaning before we can speak of the law as a self-evident proposition. As reasonably might the great law of gravity itself be called self-evident. But as a mere matter of fact Newton had no more to do with establishing the first law of motion than he had to do with establishing that which he numbers as the second, though it is more often given as the third. KEPLER established the law that *every body perseveres in a state of rest, or of uniform motion in a right line, unless it is compelled to change that state by forces impressed on it;* GALILEO established the law that *the alteration of motion is proportionate to the motive force impressed, and is made in the direction of the right line in which that force is impressed;* and NEWTON established the law that *the mutual actions of two bodies upon each other are always equal and in opposite directions,* or *to every action there is always opposed an equal reaction.*

As regards Mr. Spencer's use of words, or the nature of his definitions, it will be time enough to speak when the Edinburgh Reviewer condescends to touch the chief point of my complaint against him,—viz., that in his review he has been unjust to Mr. Spencer in assigning to him, contrary to the plain meaning of his words, doctrines so absurd as that the Almighty Being, ("touching" Whom Mr. Spencer really says, with Elihu the approved of God, "we cannot find Him out," He is Unknowable) is identical with Force, and other ideas scarcely less monstrous.—R. P.]

THE output of coal in the United Kingdom is stated to have reached, last year, the high total of 163,750,000 tons, or an increase of more than 4½ per cent. on the previous year's total. According to the late Professor Jevons' estimate, the quantity mined in 1883 should have been 178,100,000 tons, so that the actual output was 14,400,000 tons less.

Gossip.

FROM this time forwards we may hope to reduce rapidly the pressure both on our space for readable matter, and on our advertising columns. We are afraid to say how many columns of letters, notes, paragraphs, articles, &c., have been standing over for want of room.

MR. NEWTON CROSLAND'S letter appears this week as promised. I have explained that nothing I have said has been intended by way of ridicule, simply because there is nothing ridiculous in not being acquainted with particular facts or understanding special lines of reasoning. Of each one of us it must be said that even of the known we can have mastered but a most minute part, and the known is as nothing to the unknown. There are some to whom one form of not knowing seems absurd,—to wit not knowing that we do not know. But in reality this is common enough. Apart from paradoxists, whose special weakness this is, most of us are unaware how ignorant we are of some matters: and unless we have had time fully to find out, there is nothing absurd in the mistake. That Mr. Crosland does not know how little he knows of the most perfect and beautiful—most beautiful because perfect—process of reasoning by which Newton established the Law of Gravity, and applied it to explain the movements of the heavenly bodies is obvious in every line of his writing on the subject; but there is nothing to be ridiculed in this,—at least by those who, looking inwards, readjust their own readiness to fall into similar error.

To say the truth, the matter with which he deals, or tries to deal, is not so simple as it is supposed to be by most of those who have given the popular account of the matter. Sir Edmund Beckett in the preface to his "Astronomy without Mathematics" points out that some seemingly simple propositions have necessarily to be assumed in that work because there is no simple proof of them, and amongst these he mentions motion in an elliptic orbit. No wonder that when he, a passed master in the subject, treats it as not to be understood save by mathematicians, the incomplete attempts at explanation referred to by Mr. Crosland as if they were authoritative expositions fail to convince the learner.

MR. CROSLAND cannot see how velocities (and consequently centrifugal tendencies) generated by gravity can overcome gravity, how therefore a body which moving in an elliptic orbit has drawn nearer to the sun can ever pass farther from him, if the law of attraction only is at work. It does not (I suppose) suffice for him, as it does for others who have not dealt with the mathematical proof, to be told that,—in elliptic motion under gravity there is an alternation of velocity on the one hand and amount of pull on the other hand, by which first one then the other has for the time the advantage, the two being only on a par at two points on the orbit. At these two points where the planet is at its mean distance, velocity and attraction are so perfectly balanced that if only the direction of the body's motion were right (at right angles to the sun's direction) the body would move in a circle, with that velocity, and at that distance, round the sun. But as matters are, the body at one of these points is moving on a course carrying it nearer to the sun, and its velocity is being increased by the sun's pull. The centrifugal tendency grows as the square of the velocity, and also as the distance diminishes; it gains

therefore more quickly than the velocity. The angle of approach continually diminishes, then, from this point, till at last approach is converted into recession. This happens as certainly, though the velocity is increased by solar gravity, as it would if the planet were an engine, and the engineer had turned on more steam. Then as the planet recedes, at first slowly then more quickly, centrifugal tendency is diminished till at the other point where the planet is at its mean distance the centrifugal tendency is exactly such as corresponds with the velocity, and would give a circular orbit, if the body's direction were right. But at this point the body is still receding, the sun is now diminishing the planet's velocity, centrifugal tendency is diminishing as the square of the velocity, and also as the distance increases. Hence the angle of recession continually diminishes until at last recession is converted into approach. Then as the planet approaches, at first slowly then more quickly, centrifugal tendency is increased, till at length it just corresponds with the velocity, and would give a circular orbit if the body were moving in the right direction.

WHAT is prettiest in Newton's demonstration of all this (and much more) is to my mind, where he in effect takes three successive points anywhere on the body's course, and shows that given the first two points on a particular ellipse (determined by the body's known velocity in passing from one point to the other) the third point lies also on that ellipse. This he does by a very simple but pretty application of the geometry of limits. All the rest follows at once. The second and third point lying on that ellipse so does the fourth; so the fifth; the sixth; the seventh; and so forth, to infinity. Nor does the mathematician need to be told that though the proof does not and cannot deal with really successive points, and though, if it did, no number of successive points would give a finite part of the orbit, the proof is mathematically perfect. It shows in effect that (1) let the three points first dealt with be ever so near they will lie on the ellipse, and (2) that any number, however great, of these ever-so-near points may be taken without the ellipse being departed from. Wherefore the ellipse will be traversed, for ever,—unless external disturbing forces intervene.

I HAVE been a good deal surprised, and so has Mr. Thomas Foster, to find that several correspondents (some of whom are not of the cavilling and ill-reasoning kind referred to by him in a foot-note in last number of KNOWLEDGE) misjudge entirely the gist and purport of his papers on the Morality of Happiness. They seem to imagine that the question of Conduct cannot possibly be considered without reference to Religion,—not to Religion in its stricter verbal sense, but to Religion as commonly understood by the word. Now of course there can be no manner of doubt that rules of conduct are regarded by an immense number of persons as essentially associated with religious doctrines. But it is equally indisputable that there is no *necessary* connection. How much or how little of conduct may be regarded as depending on religious doctrine is a question I need not consider. It is certain that *some* parts of conduct are with at least nine hundred and ninety-nine out of a thousand not governed by religious considerations at all. Mr. Foster at the very outset repudiated all reference to the religious element in regard to conduct. He set out by showing how conduct has been evolved in the struggle for life, how it continues to be evolved, and manifestly will be evolved hereafter, in such sort as to make conduct increasing the totality and fulness of life prevail in the long run over conduct which exercises a contrary effect. Then he showed how the totality and fulness of light are related to happiness. He is now engaged in showing how essential to the general well-being is due care of personal well-being. And he will show later how essential is care of the interests of others, not only for social well-being, but personal well-being also. If the line of conduct thus indicated as *right*, and the line of conduct which we are thus led to deprecate as *wrong*, be found to be closely akin to those approved or deprecated in former times with the sanction of what has been regarded as revelation (I am speaking here in the widest sense, so as to include much that *every one* of *every* religious persuasion would deny, and *does deny*, to be revealed) well and good: but it is no part of Mr. Foster's purpose to consider this point. Neither is it any part of his purpose to call in question any belief held by the members of any church or sect whatsoever. He simply indicates those lines of conduct which promise increase of general happiness, and those on the other hand which threaten the reverse. Among the multitudinous teachings of past ages, under different conditions, and in times when either much sterner or much milder conduct may have better suited the surroundings, and therefore have been best, are doubtless some agreeing with and some not according with those which seem best now. But this has nothing to do with Mr. Foster's purpose here, any more than—it should seem—with Mr. Herbert Spencer's wider teaching. The subject as treated by Mr. Foster is purely scientific, not religious at all, and he declines absolutely (with my sanction, as he would have declined at my request had not his view been the same as mine) to pursue any of the religious discussions which some correspondents have desired to raise over his scientific analysis of the Morality of Happiness.

BUT perhaps the very title Morality of Happiness has been by many misunderstood. Does any one by any possibility imagine that Mr. Foster's subject is the question whether it is moral or immoral to be happy? It would seem so, though the idea really appears too absurd to be entertained. Does Mr. Foster's reasoning read as if he wished to discuss a question so idle and so puerile? His subject really is the Morality of Happiness,—that is, he is inquiring what is the course of conduct indicated when happiness is taken as the guide, when the well-being of the body social and therewith the increase of the fulness of its life, is regarded as determining individual conduct. In other words Mr. Foster's question is,—*not*, Is there Morality in Happiness? but What are the moral rules suggested when we take general happiness as the guide of conduct?

THE mistake of several correspondents in regarding *this* as a religious subject reminds me of a story which some of our readers may not have heard:—A Highlandman asks a companion the truly perplexing question, "Say, Donald, is a pumple-pee a pird or a peast?" After some vain cogitation, Donald replies rather angrily, as is the way in such cases, "Eh, mon, ye mauna trouble folk with releegious questions on ilka day." ("Ilka day" or "each day" being here equivalent to "a week day."

LET me assure anxious readers that neither Mr. Foster nor any one else proposes "to trouble folk with releegious questions" in our professedly weekday magazine.

WHAT *has* been the matter lately with the erst genial *Punch*? It seems to find the present age so far inferior

to all past time that it is calling aloud—though vainly—for "the Muse that laughed and stung on Gulliver's indignant tongue,"—a very Billingsgate Muse—to denounce the coarseness and vulgarity of the age. A homœopathic remedy truly, though not a curing of like by like infinitesimal doses, but by doses such as our age would hardly swallow. Does the age really merit Mr. Burnand's accusation? Is it really so much worse than past ages that nothing short of the venom (strong poison, but still poison) of Swift as he neared the doom of long-threatened madness, can avail to correct its evil ways? What does the would-be Swift, the "stifled spirit" as he calls himself (with probable truth), the "vexed soul," find now that is worse than in the days when Pope showed us in the Dunciad what were the literary men of his time and how in his time they might be rebuked without offending the taste of his day? He finds "the statesman's cut and dried abuse:" this is not new, if the satire of past ages be believed. "The lawyer's pæans in his fees, and actor's noisy juggleries"; the critic, "*sans* scholarness or kindliness"; "patriots for bright sword carrying fish-fag's tongue": why, all this was said, but better, in the good days of old. Then Science has its turn, the biologist being taken as the typical wretch, "probing with his two-foot rod his muddy substitutes for God," though it takes an essentially unscientific eye to see anything like this in poor dear protoplasm. Lastly we are told, in lines meant to be fine, though in truth little better than windy nonsense, that we now have

> All our sires held first and best
> In pufferies of all sizes dressed,—
> Till England watch through England's press
> The fall of English manliness.

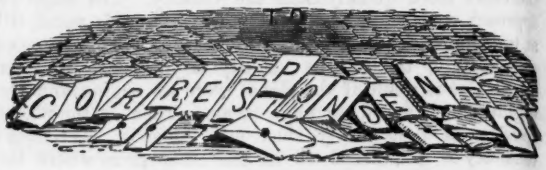
The only feature of the time which may be regarded as apparently uglier than of yore, might be worthily castigated in the pages of *Punch*. Our modern Juvenal who cries for "some Juvenalian verse," "which Nature" (as he truly says of his) "may desire in vain" has but feeble lash strokes for this foul evil, and expresses "in English carefully destroyed" his anger against

> Those who serve the weekly meal
> For jaded gluttons, keen to gloat
> On savoury sauce of Anecdote.

His picture of keen jaded gluttons gloating on sauce will not go far we fear to correct the evil of which our Society journals are the mere symptoms, not the cause. Students of the literature of the eighteenth century know that the evil existed then as now, though not then so public.

IN passing we note that even in speaking of the memory of the well-loved Calverley, our contemporary shows the same touch of jaundice. Where only pleasant memories need have been evoked, he ponderously brings down his truncheon on "ponderous posers," "prigs and bores," "sombre wisdom," "the Big Bore race," "severe quidnuncs," and so forth—and is actually at the pains to call the attention of the "much-to-be-pitied Grand Panjandrums of Useless Knowledge" (ponderous prosing this) to Mr. Calverley's Examination Paper in "Pickwick." How it may be with Grand Panjandrums we know not; how it is with the author of this jaundiced tribute we can guess: but we should say that certainly scarce any others need to be reminded of so well-known a paper.

THACKERAY says somewhere that a man is a bore when he is bored: we fear Mr. Burnand has been badly bored of late.



"Let Knowledge grow from more to more."—ALFRED TENNYSON.

Only a small proportion of Letters received can possibly be inserted. Correspondents must not be offended, therefore, should their letters not appear.

All Editorial communications should be addressed to the EDITOR OF KNOWLEDGE; *all Business communications to the* PUBLISHERS, *at the Office, 74, Great Queen-street, W.C.* IF THIS IS NOT ATTENDED TO DELAYS ARISE FOR WHICH THE EDITOR IS NOT RESPONSIBLE.

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WORKS ON BOTANY.

[1129]—T. Garth wishes to know the name of "a cheap but trustworthy book" on botany, with which to follow my papers in KNOWLEDGE. This is rather a vague request, for everything depends upon whether he wants a book on general botany or one on the British wild flowers only. For the flower, I should recommend Oliver's "Lessons in Elementary Botany" (Macmillan, 4s. 6d.); for the latter, either Bentham's "British Flora" (Lovell Reeve, 12s.), or Hooker's "Student's Flora of the British Islands" (Macmillan, 10s. 6d.). Bentham is easier for beginners, and has an artificial key; Hooker is fuller, more technical, and more advanced. If T. G. has access to any library containing Syme's "English Botany," with (Sowerby's large coloured plates (a big and expensive book in eleven volumes) he will do well to follow the papers with the illustrations there. So-called "popular" works, in which a few common species are ill described, ought to be avoided. Better go to half-a-guinea, and buy Hooker while one is about it. As to a lens, a sufficiently good one may be got for six or seven shillings. But here, too, one of Mr. Browning's platyscopics (lowest power best for a beginner) will give far more satisfaction, and prove a better investment in the long run; the price (I believe) is 18s.

GRANT ALLEN.

THE "NEW PRINCIPIA."

[1130]—I am quite aware that you can, in your own columns, make me appear as ignorant and ridiculous as you please. I have no power to check this centrifugal force of your assumptions and arguments, which fly off at a tangent wherever I venture to control them. I quite "understand" why the centrifugal force of an engine on a railway "varies in potency;" it is owing to the guidance of the driver acting on obedient machinery; but no such explanation is available in the case of a planet moving in a curve round its primary. Newton and his disciples contend that the *acceleration* of speed, caused by gravity, develops a centrifugal "force" supplementary to a direct motion already assumed, which "force," in its turn, practically overcomes its own cause! I have shown, in the "New Principia," that this hypothesis is self-contradictory and impossible in reality; whereas, according to my theory, the variability in the motion of the planet is due to the variation in its polarity, the slanting direction of the axis and the presentation of a different pole to the sun being sufficient to account for the motion of the planet and the fluctuating of this motion. In the case of the engine on the railway, mentioned by you, if, when left to itself, without the aid of the driver, it changed its centrifugal tendency whenever it was prudent to do so, we should then be obliged to search for this skilful adaptation in some automatic arrangement. In the case of the planet, I find this automatic device in the attraction and repulsion of polarity; and no other explanation of the movements of the planets seems to me to be satisfactory. Every schoolboy knows that a body having once acquired a motion in a straight line has a tendency to continue in the same direction.

I rely upon you setting me right with your readers.

NEWTON CROSLAND.

GHOSTLY VISITANTS.

[1131]—The book you refer to in your article on ghosts is, I think, "The Night Side of Nature;" but my recollection of the circumstance is somewhat different from yours. I think that the authoress (?) said that Sir Walter was passing through his *entrance-hall* when he thought he saw Byron standing on the opposite wall, and, after vainly trying to dispel the illusion where he stood, he walked up to find a gentleman's *cloak* hanging on a hat-peg.

I lately heard a well-authenticated story of a "vision" connected with a family in this neighbourhood. One of the sons was an officer in the army before Sebastopol. One night another son, at home, thought he saw his brother standing in his room, his uniform smeared with blood flowing from several wounds in his breast. Next morning he acquainted his family with what he had seen, adding that he felt sure that his brother was killed.

The next mail brought a letter from the colonel of the regiment, informing the father that his son had been killed in the trenches on the very night when the spectre appeared to his brother.

After the war the regiment returned to England, and one day the brother was invited to dine at the officers' mess. Naturally, they began to talk of their comrade's death, when the young man interrupted the narration by saying, "Stop! Tell me; were his wounds here, and here, and here?" pointing to his own breast. One of the officers said, "How do you know this?" "Wait," he said; "am I right?" "You have described the places exactly," replied the officer. The young man then told them what he had seen.

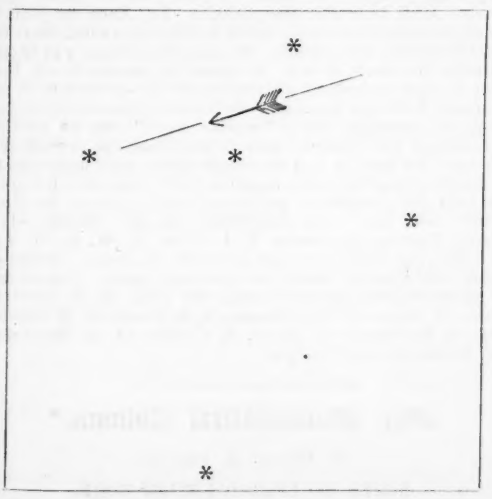
I have heard another equally well-authenticated case, of a young man in Valparaiso seeing the appearance of his mother (whom he had left in Scotland) twice in one night, of his noting the fact next morning on a sheet of paper, which he handed sealed to a friend, and of his learning by next mail that his mother had died that night, as he had feared. His nerves were so much shaken by the circumstance, that he gave up his situation and returned home.

An intimate friend of mine assured me that he had the story from the young man's own lips immediately on his return.

Broughty Ferry, *Feb.* 16, 1884. SENEX.

METEOR.—RED GLARE.

[1132]—Large meteor seen here (Pornic, Loire-Inférieure) a few minutes past 8 p.m. Dec. 19, in Cygnus as per sketch. Light blue; lit up everything like moon five days old; left trabs for few



seconds. 17th, about 6.15 p.m. (Paris time), I noticed the Zodiacal light for the first time this winter; earlier than usual, but not so early as last year; faint, but distinct. Sunday evening not so clear, and Zodiacal light hardly visible at all. The red sunsets, green moon, and lamps have been very remarkable here, since Nov. 30 especially. I have had a keen eye on the skies for forty years, and never saw anything like them before, especially in this respect. From one window (N.W.) is pitchy black, with nimbus (16th); I look from the other (in the same wall) to S.W., and meet a blaze of crimson light, which pains the eyes, night having fallen. Now, *usually* a red west would include *all* the west. A squall and shower succeeded, as if the phenomenon were wholly terrestrial, whereas the localisation of the glare over the sun's place would almost suggest a more cosmical origin than as explained by Mr. Ranyard, *i.e.*, that the Z.L. (supposed to extend beyond the orbit of Venus?) had this year from some cause come as far as our own. That night (16th) there was up to 11 p.m. a brightness in the west, which was not visible over the horizon from south to north-by-west, except these, in a region not opposite the moon, then shining, but to the south of the sun's place.

On the 17th, when night had practically begun, the effulgence of the west made my eyes ache, not red, but yellowish white. Just after sunset the whole west sky was streaked with milky bands, all radiating from the sun, but not like clouds. They looked like masses of matter. They must have been there before sunset, but were then invisible. They were not rays of light, being not perfectly straight and definite. I never saw such before. A procession of small nimbi were sailing rather rapidly from north to west (wind north-east), so these fleecy pencillings must have been much higher up, being motionless. HALLYARDS.

MIGRATION.

[1133]—"Naturalist" hits the weak point in Mr. Grant Allen's theory of migration exactly, when he says that it will not apply to small birds. He might have stated further, and with equal truth, that there are very few birds it will apply to. Mr. G. Allen generalises too much; he constantly speaks of "the bird," as if all birds, or, at any rate, all migrants, were much the same in their habits and powers of flight.

He contrasts our slow and tortuous methods of getting from one point to another with "the bird's" free and easy plan of rising high enough in the air to see his destination, and sailing straight for it. The very idea of, say, a willow or golden-crested wren, or any, indeed, of our warblers, soaring up in the air like a skylark and making a "bee-line" for some object, is, to any one who has at all studied the way of birds, so absurd that it is hard not to think that Mr. Allen must have forgotten how large and varied a number of our "feathered friends" are birds of passage.

Now, in reality, birds travel much as we do, only they use hedges, and especially watercourses, as we do roads; they are, in fact, their highways. Birds of all kinds will follow rivers for days, and flocks of swallows have been seen to fly for miles along the seashore itself. (Perhaps they were looking for a "landmark?")

Then, again, birds constantly travel by night. Our summer visitors usually arrive in early morning. This I have known to be the case on our east coast. How about the "landmarks" in this case?

Young birds, too—last year's nestlings—often arrive by themselves, some days after the older birds. This is a well-known fact in the case of some of our Norwegian visitors. These young travellers could know no landmarks.

But though Mr. G. Allen's theory can scarcely apply to all, or even to many, of our migrants, it may to some, as the vast tribe of swallows, of which, possibly, he was chiefly thinking; and yet even here I may be allowed to doubt that the most sagacious swallow can "spot" Beachy Head from Boulogne "as the tallest white cliff," or recognise St. Martha's Down by its "high-perched chapel tower." Still, this may be so. C. COURTENAY HODGSON.

CAVERNS IN THE EARTH AND MOON.

[1134]—Perhaps you will allow me as one who knows the neighbourhood to correct the quotation from Mr. Mattieu Williams, relative to the caverns in the counties of Galway and Mayo. In the first place, the river joining Lough Mask and Lough Conn ought to be the river joining Lough Mask and Lough *Corrib*. In the second place, I doubt if it is correct to speak of this river as deriving its corroding powers from its passage through a peat-bog. There is no peat-bog between the lakes. The upper lake (Lough Mask) is fed by a number of streams, some of which, no doubt, pass through peat-bogs, but the principal tributaries enter the lake at a considerable distance from the point where the water leaves for Lough Corrib, and do not enter it directly from peat-bogs.

The extent to which the limestone rocks of this district are caverned by water can only be known by those who visit it in winter. I am inclined to think that the honey-combing stretches across the country for fully twenty miles, the two most remarkable phenomena marking its limits. One of these is the disappearance of the river connecting Lough Mask and Lough Corrib, the other is the disappearance of the Turloughmore river for a distance of fully three miles (I believe) between Corofin and Claregalway. In both places some water runs overground during the winter, the disappearance being complete in summer. Between the two rivers a third, known as the Blackwater, which passes Shrüle and runs near Headford, exhibits similar disappearances on a small scale, and I have a suspicion that

some of the rises of water near Headford belong to the Turloughmore river instead of the Blackwater. A small stream which falls into Lough Corrib near the little village of Cross, between Cong and Headford, shows the same characters, and a rise which takes place just below the village of Cross is probably connected with Lough Mask, though the stream comes from a very different direction. In winter, water (usually from Lough Mask) bursts up in numerous places, which a summer visitor would never dream of—the outburst sometimes amounting to a considerable river, which again disappears after a very short course. In other places it forms a small temporary lake, which becomes dry in spring. It was, I believe, the outburst of water from beneath which led to the abandonment of the canal to which Mr. Williams refers. It seems evident that fish can pass freely through these underground caverns.—I remain, &c.,

W. H. S. MONCK.

Dublin, Dec. 22, 1883.

THE FIRST INVENTOR OF THE TELEPHONE.

[1135]—An article headed thus in KNOWLEDGE, Vol. IV., No. 108, reminds one of a very old, but a very true proverb, "that there is nothing new under the sun." This applies to the telephone as well as to many other things.

The *speaking telephone* (not the electric one, of course), may have been *re*-discovered by Philip Reis and others, and, in that sense, be an original invention; but it has been known for some generations, if not longer, to the natives of India, as the following story will show:—

When the speaking telephone was first talked of and began to be heard of in India, a friend who then held an official position in the Punjaub spoke of this then wonderful new invention to his bearer, a native servant who had been with him some years.

The man replied, "But, Sahib, this is a thing which we natives know very well; I and a fellow-servant use it daily in your compound when we wish to talk to each other across it from our godowns" (a native servant's room is thus called).

His master, very much surprised, asked to see this instrument, which was afterwards produced for our benefit also; it consisted of two hollow pieces of bamboo, each about 8 in. in length, and closed at one end with a piece of parchment, through which was passed a string some 50 to 100 ft. in length; it perfectly answered the purpose for which it was intended.

I have been told that the natives of Ceylon use a similar apparatus to enable them to communicate with each other across the deep valleys in that island.

COSMOPOLITAN.

LETTERS RECEIVED AND SHORT ANSWERS.

OBTUSE.—Not obtuse but acute and right. The hour is later the farther east the place is, not the other way as Dr. Brewer puts it.—J. PARRY. "Quite too awfully exactly described."—E. WAL WARMINGTON. The subjects you object to are not objected to by quite so many as you seem to think, but quite the contrary. Just before your letter four others expressing the contrary opinion were opened. As to what you propose you must please yourself, and allow us to try to please the great majority. Yours is the one solitary letter expressing that view out of about the last two hundred received. If 1 in a company of 200, or 5 in a company of 1,000, told the host to "shut up" as you politely tell us, while the rest wished him and many specially asked him to go on, do you suppose he would view with *very* great pain a threat of withdrawal on the part of what could hardly be called even a minority (but rather, simply "a disturbing element which had no business in the company")?—H. B. You are quite right. There is not the slightest reason for supposing that the sun shining on a fire puts it out. You might easily miss the reference to the matter in early numbers of KNOWLEDGE, as it was under a different heading. It was in an article of mine on Fallacies I think that the question was touched on. Yesterday I watched with some interest the vigorous burning of a fire in an open fire-basket in the full blaze of sunlight. It *looked* dull enough, because the sun's light was so strong; but it needed only a little watching to show that the fire was full of vigorous life: and the men close by who used the fire for their work were manifestly not troubled by any fear that the sun would put it out. Dr. Brewer beautifully explains the matter somewhat like this; The rays of the sun put out a fire because they act unfavourably on the process of combustion.—CANIS MINOR. 1. In an astronomical telescope the north pole of Mars (as seen from northern places) appears below the southern. 2. In the southern hemisphere the reverse happens. 3. I suppose my reply to 2 *does* bear on the flat-earth theory; but it is impossible to say *what* does or does not bear on a theory whose only positive quality is absurdity. 4. I imagine the reason why in most maps the north part is put uppermost is that at our northern stations our heads are directed more nearly towards the north pole of the heavens than the south.—A. F. OSBORNE. Thanks; oddly enough, the day I had sent off that reply I saw Anster's translation (published by Messrs. Routledge) on a railway bookstall. Mr. Spencer's statement was made to Professor Fiske, and published in America several years ago.—J. T. PEERS. It has been somewhat blatantly *asserted*—but not by any one competent to form an opinion—that the Gulf Stream does not appreciably affect our climate. You will scarcely find any "authority" saying so.—F. W. HALFPENNY. Doubt much whether the numerous ladies we count among our readers would altogether appreciate your suggestion that they should take to smoking in order that their minds may be turned to more serious matter.—ERNEST EVANS. Thanks.—W. THOMSON. Do not know.—A. G. BIGGS.—S. M. B. They must be badly bound, it is not so in any of my volumes.—L. ALLEN. Thanks.—J. B. ADAMS. Thanks for amusing parody, which should appear but that I fear to wrong some who love the original even as I have myself suffered from parodies of well loved lines.—J. H. C. Yes, certainly, the parts of tire nearer the ground move more slowly in space though they go round the centre at the same rate.—M. J. HARDING. Your letter reached me after the number for Feb. 29 was already arranged. Thanks for suggestions in your other letter. Your suggestion, that my "Gossip" should *commence* each number, comes oddly soon after a suggestion by a rather ill-mannered correspondent that it should, on no account, appear at all.—C. CARUS WILSON. I know of no remedy for the spots which have formed on the O. G. of your telescope. The glass will not be much the worse for them unless they cover much of the surface.—W. WALTERS. Do not know the work; but should think it very probable De Morgan wrote it, as he did many for that series.—E. K. HALL. It is quite evident your corrections of the account of the motion of the terminator at p. 222, Vol. III., are needed (and one more, 38′ for 37′ in third statement)—*i.e.*, daily motion, 12° 11′ 26″·7; longitude of central point at end of first day, 77° 49′ 33″·3; of second day, 65° 38′ 6″·6.—J. J. POLLOCK. There is such a line, or rather circle; and if on one side of the line it is Wednesday, on the other it is Thursday. An "insurmountable anomaly" you call this; but not really much more so than the existence of a different instant at two places when Wednesday changes to Thursday. For instance, when Thursday has begun with us in England it is still Wednesday with you in Ireland.—THOS. PEARSON. There is but one way in such cases: send article on approval with stamped and directed cover for return if not approved.—J. W. STANIFORTH. We owe you an apology; your name was, we remember, most clearly written.—H. B. LINDSAY. Many thanks. The lines are beautiful, though they would be *caviare* to the dullards who asked what Vega meant by making a sun speak. Albeit I object strongly to Goethe's invitation to *comets* to roll. It cannot be permitted.—E. PLATT. Nay; if there is a subject we should not care to touch on in these columns it is the one you name. It is one of those on which, as on many, we personally take a "median way;" and we have long since learned that however true in one sense the proverb *medio tutissimus iter* may be, it is in another sense most untrustworthy. A traveller along the middle way finds himself saluted with missiles from both sides,—meant to go across the way perhaps, but if they "fetch" *him, that* is no consolation.—G. M. Thanks.—P. DE HAMEL. Thanks.—EXCELSIOR, T. L. HALL, G. W. G., H. A. B. Mean time, not solar time.—AN AMATEUR, W. POOLE. Properly to discuss your theories would take too much space. Cannot under any circumstances answer through the post.—H. D. CUTBILL.—ARIEL.—E. GROTT.—C. F. C. NOAD.—R. M. PRATT.—J. H. GARFIT.—J. M.—J. B. PEARSE.—J. L.—D. B. CAZAUX.—J. E. MACKLAND.—T. B. BATTERSBY.—R. PHILLIPS.

Our Mathematical Column.*

BY RICHARD A. PROCTOR.

NOTES ON EUCLID'S FIRST BOOK.

WITH SPECIAL REFERENCE TO THE SOLUTION OF GEOMETRICAL PROBLEMS.

THE First Book of Euclid treats chiefly of the properties of triangles and parallelograms. An examination of the book suffices to show that Euclid had proposed a definite line of treatment leading up to certain important propositions. Hence many

* As the Editor has been repeatedly requested to give only in this column what is easily followed and likely to be useful to the general student, he has decided to give a series of papers on the successive books of Euclid, accompanied by a series of easy riders on the several propositions of the different books. On alternate weeks will appear "Easy Lessons in Co-ordinate Geometry."

useful properties are left untouched in this book. It is surprising however how many valuable propositions Euclid has succeeded—by a judicious method of treatment—in introducing into his plan without marring its symmetry.

In attacking deductions either immediately depending on the First Book of Euclid or involving it in part only, it is necessary that the student should have at his fingers' ends, so to speak, all the most useful properties established in the First Book, and also several important properties deducible from this book. We proceed to examine the most valuable of these.

In the first place, let us run through the first book and notice whether there are any properties whose converse theorems, though not proved in Euclid, may be readily established.

Euclid has proved the converse of prop. 5 in prop. 6, of prop. 13 in prop. 14, of prop. 18 in prop. 19, of prop. 24 in prop. 25, of props. 27 and 28 in prop. 29, of prop. 37 in prop. 39, of prop. 38 in prop. 40, and of prop. 47 in prop. 48. The other propositions which admit of a converse are the following:—

Euc. I., 15, of which the converse is, "If two straight lines C E, D E (Fig. 1) on opposite sides of a line A B, make equal angles C E A,

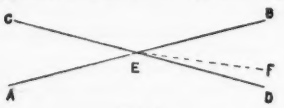


Fig. 1.

D E B with A B; then C E and E D are in the same straight line. This is obviously true, since—if C E produced fell in some other direction as E F, we should have the angle B E F equal to the vertical angle C E A, and therefore to the angle B E D, which is absurd. We shall refer to this proposition as Euc. Bk. I., prop. 15, *Conv.*

Euc. I., 17. The converse of prop. 17 is Axiom 12. We touch here on the great defect of Book I., a defect, however, with which our subject does not lead us to deal.

Euc. I., 34.—This proposition contains three theorems, each of which has a converse, but the converse of the third is not true. The converse of the first part of the proposition is this:—*If the opposite sides of a four-sided rectilinear figure are equal, the figure is a parallelogram.* This is obviously true; for, having A B (Fig. 2)

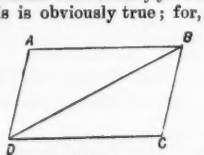


Fig. 2.

equal to D C and A D equal to B C, also the base B D common, we have the angle B A D equal to the angle B C D, Euc. I., 8, and the triangles B A D, B C D equal in all respects, Euc. I., 4, so that, the angle A B D being equal to the angle B D C, A B is parallel to D C, and similarly, A D is parallel to B C. We shall refer to this proposition as Euc. I., 34, i. *Conv.*

The converse to the second part is also true. It is,—*If the opposite angles of a four-sided rectilinear figure are equal, the figure is a parallelogram.* In this case, having the angle D A B (Fig. 3) equal to

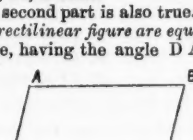


Fig. 3.

D C B, and A B C to A D C, we have the sum of the angles D A B and A B C equal to the sum of the angles A D C and D C B, or either sum equal to half the sum of the four interior angles of the figure; that is, to two right angles Euc. I., 32, cor. 1: hence (Euc. I., 28) A D is parallel to B C., and similarly A B is parallel to D C. We shall refer to this proposition as Euc. I., 34, ii. *Conv.*

The converse of the third part of the proposition is not a true theorem, for it is clear that besides D C B (Fig. 2) an infinite number of triangles may be drawn on the base B D equal to A B D, each of which would give a quadrilateral divided by B D into two equal triangles, but this quadrilateral would not be a parallelogram.

Euc. I., 35 and 36.—Each of these propositions has a converse which may be established in the manner of props. 39 and 40. We shall refer to these converse theorems as Euc. I., 35 and 36, *Conv.*

Euc. I., 43, has the following converse,—If *H K G* (Fig. 4) *is drawn parallel to the two sides A E, D C of a parallelogram A B C D and E K F parallel to the other two sides, in such a manner that the parallelogram H F is equal to E G, then H F and E G are complements about the diagonal A C,—in other words the point K lies on the diagonal A C.* This may readily be proved to be true. For, if A C

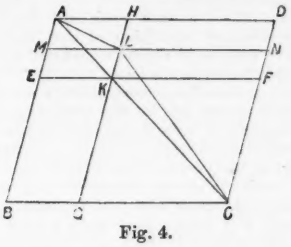


Fig. 4.

had some other position as A L C, then drawing M L N parallel to A D or B C we have the complement B L equal to the complement L D; therefore B K is less than L D: but B K is equal to K D; therefore K D is less than L D, the less than the greater, which is absurd. Hence B K and K D are complementary parallelograms,—or K is a point in the diagonal A C. We shall refer to this proposition as Euc. I. 43, *Conv.*

(To be continued.)

Our Whist Column.

By "Five of Clubs."

A WHIST GEM.*

THERE are some very excellent rules at Whist, and others not quite so good, which are too often held by average Whist-players as general rules. The idea is that if any exception exist at all to these rules, it is only when some "passed master" of Whist sees the way to a *grand coup*. For ordinary players these rules are supposed to be practically universal. The result is that average players steadily obey these rules, which are not general at all, to the discomfiture of the best laid plans of a partner of wider experience and deeper insight.

We lighted, however, the other day on the book before us, a little work sold freely at railway stations and elsewhere, which besides laying down several of the good old rules as general (though they are open to numerous exceptions), gives a number of perfectly preposterous rules as valid. Let us cull and comment on a few of its remarks.

"Only lead Ace from Ace and five others,"—misprint for "four others." A rule almost universal, but did not Mr. Lewis show recently in our columns how even as an original lead the penultimate may occasionally be the right card? However, let this rule stand as advisedly a general one.

"Wherever you can as fourth player, only make the trick with the Ace, play it. Never pass an Ace trick in order to keep the lead." On the contrary it repeatedly happens that keeping back the Ace is essential to success in the strategy of the hand,—apart from cases, by no means infrequent, where it is obvious that if you play the Ace you make no other trick, while if you hold it up you make two tricks.

Here is an odd general rule, but odd only because of bad English: what the writer means is sound enough:—"Draw a conclusion and adhere to it until the play proves it wrong: then proceed again." It would be pleasant to have a partner who was always forming wrong conclusions and acting on them till they were proved wrong, then beginning again, and so forth. For "until" read "unless" and all is pretty well.

"Always throw out a forcing card, when, whatever the loss to you, the loss to the enemy must be greater." Here probably the author of this remarkable book should have written "if" for "when," or omitted the comma before "when." With the comma the rule implies that every "force" is good. The rule correctly given admits of being made much more general in the form, "Always be ready to sacrifice a trick if you can gain two or more by so doing." This is an excellent rule, and would be as useful as sound, if it only showed *how*.

"The golden rules in Whist are (1) lead the lowest of a strong suit, (2) the highest of a weak suit." Consequently from Ace,

* "Whist." By J. R. W. Warne's Bijou Books. (London: Warne & Co., Strand.)

King, three small ones,—plain suit,—*always* lead a small one; and from King two small ones, *always* lead the King: you have no idea how pleasing the effect will be on a choleric partner.

"Never lead from two plain cards while you have more of another suit:" not even if you know, for instance, that your partner must be strong in the suit to save game, and that leading from any other suit must destroy that chance.

"Where you must lead from a weak suit, lead invariably the highest"—an unnecessary repetition of that second golden rule which, as we have suggested above, would make your partner so cheerful if always followed to the letter.

"When a suit is at once the second player's strong suit, and the fourth's weak, by all means lead it: the advantage to your partner is great;" for instance, if second player holds the master-card in the suit, has signalled for trumps, and having secured command in trumps can bring in his partner's established suit, the advantage to your partner must be great; for he will be saved all further trouble, and so will you. Now had you perhaps not followed the law thus laid down, you might have made a trick or two in your own hand and your partner's suits, and positively have had the trouble of playing another hand before the game was quite over.

But when we turn to another page and find *this* rule, "In all suits, trumps apart, only lead King when you have *Queen* and *Knave*," we find it only necessary to ask in conclusion, What sort of Whist players are we likely to meet if Messrs. Warne's "Bijou Whist" is very widely read and very carefully followed?

ANSWERS TO CORRESPONDENTS.

Correct solutions of Endings, p. 62, received from A. S. Orr, Cucumber, E. F. Lewin, J. N. Prang, and E. Le Mesurier; of 2nd (1st already correctly solved), from Q. T. V.; of 1st, from A. Jenkins, Emily Carmichael, Club Knave, and Cucumber.

Q. T. V.—Even if position could not have arisen in play, it might still resemble a Chess problem. I have seen a problem in which a King's Bishop was afield though King's Pawn and KKt's pawn unmoved. Who says that Y-Z had won all the other tricks? See illustrative game last week, which was in one striking point like the ending. Yet it occurred in play several months since. Its occurrence in play led one of the company to remark that he remembered something like it in an ending of Mr. Lewis's: and this having been hunted out we thought worth quoting here.

W. G. NICKLES.—Counting at short whist is simple enough. The side which first makes five points wins game, which counts as *three* if other side have made no points, as *two* if they have made no more than two points, and as *one* if they have made more than two. The rubber is won by the side which first wins two games. The scoring for the rubber is obtained by adding *two* to the points for the two won games, and deducting the points for the game lost, if any.

IN answer to many correspondents we note that of course there was an erratum in the reply at p. 105, that at long whist the question, "Can you One, partner?" cannot be asked until the first trick is completed. The words which follow show that the "not" should have been omitted. We were indicating up to what moment the question can be put.—FIVE OF CLUBS.

Our Chess Column.

AN UNSOUND BUT AMUSING GAME.

THE following game illustrates the lesson which our brilliant Chess Editor has taught, that the armour of Saul though good in itself may not suit the stripling warrior. Black had been learning the openings, and also the general rules for the conduct of a game. In guiding my play against him, I took at once his knowledge and his want of knowledge into account. Had he not shown a proclivity for certain little weaknesses I should never have ventured on the line of play pursued in the little game which follows. Even if I had had the aid of Queen's Rook, how Mephisto would have turned the attack against me after the 8th move, if I could conceivably have tried such an experiment on him or indeed on any full-grown Chess warrior!—

KING'S BISHOP'S GAMBIT.

Remove White's QR.

White. R. A. Proctor.	Black. Another Amateur.	White. R. A. Proctor.	Black. Another Amateur.
1. P to K4	P to K4	6. Kt to QB3	B to Kt2
3. P to KB4	P takes P	7. P to Q4	Kt to K2
3. B to QB4	P to Q4	8. B tks BP (ch) (*a*)	K takes B
4. B takes P	Q to KR5 (ch)	9. Kt to KB3	Q to KKt5
5. K to B sq	P to KKt4	10. P to KR3	Q to Q2 (*b*)
11. Kt tks P (ch)	K to K sq	20. K to K sq	Q takes Kt
12. Q to KR5 (ch)	Kt to Kt3	21. Q to KB6 (ch) (*g*)	Q to B2
13. Kt takes RP	Q to KB2	22. Q takes P	Q takes Q
14. Kt to Q5 (*c*)	Kt to QR3	23. B takes Q	B to K2
15. P to QB3 (*d*)	Kt to K2	24. R to KB sq	K to Kt2
16. QKt to B6 (ch)	B takes Kt (*e*)	25. B to K5 (ch)	K to Kt3
17. Kt tks B (ch)	K to B sq	26. R to KB6 (ch)	K to R4
18. Q tks R (ch)	Kt to Kt sq	27. K to KB2	P to QB3
19. Kt takes Kt (*f*)	Q to QB5 (ch)	28. K to KB3	R to QB sq (*h*)
		29. White mated in four (*i*)	

(*a*) Up to this point the moves have been "book"; White's 8th move is a novelty and a tolerably unsound one.

(*b*) Queen falls if she goes anywhere else.

(*c*) Unsound; but White *knew* from experience that Black would reply as he did.

(*d*) Also here, that Black would play to dislodge the QKt and proffer exchange of Queens.

(*e*) Or Queen falls.

(*f*) Far better take the Kt at once: White usefully displaces his King, and gains a move.

(*g*) To improve his position before exchanging Queens.

(*h*) Playing near the precipice. Black had pushed on his King with the idea that he was developing his game. In moves 28 and 29, his object has been to bring his Q Kt and Rook into play. White's object in advancing his K to B3 is entirely overlooked.

(*i*) 29. P to Kt4 (ch) / K to Kt4 — 30. P to R4 (ch) / K takes P — 31. R to R6 (ch) / K to Kt4 — 32. B to B4. / Mated.

R. A. P.

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